DOCUMENT RESUME

ED 143 854　　95　　CE 012 481

AUTHOR	Jencks, Christopher; Rainwater, Lee
TITLE	The Effects of Family Background, Test Scores, Personality Traits and Education on Economic Success.
INSTITUTION	Center for the Study of Public Policy, Cambridge, Mass.
SPONS AGENCY	Employment and Training Administration (DOL), Washington, D.C.; National Inst. of Education (DHEW), Washington, D.C.
PUB DATE	Apr 77
GRANT	NIE-G-74-0077
NOTE	883p.; For a related document see CE 012 476. Several charts and pages may not reproduce well due to faint type
AVAILABLE FROM	National Technical Information Service, Springfield, Virginia 22151
EDRS PRICE	MF-$1.67 HC-$47.55 Plus Postage.
DESCRIPTORS	*Academic Achievement; Cognitive Tests; Demography; *Economic Status; *Educational Experience; *Employment Level; *Family Background; Income; *Individual Characteristics; Males; National Surveys; Occupations; Personality; Personality Tests; Racial Differences; Research Methodology; Social Indicators; Social Science Research; Socioeconomic Status; Statistical Analysis; Test Results

ABSTRACT

Ten surveys of American men aged 25-64 were analyzed to determine the effects of family background, adolescent personality traits, cognitive test scores, and years of schooling on occupational status and earnings in maturity. Some of the findings follow: Data on brothers indicated that prior research has underestimated the effect of family background on earnings. Adolescent test scores indicated that cognitive skills have a substantial effect on occupational status and earnings independent of background. Data on adolescent behavior indicated that personality traits may exert as much impact on economic success as cognitive skills. Controlling background and adolescent test scores indicated that less than half the observed association between years of schooling and earnings is causal. (The last third of this report covers the study's methodology. It examines the measures used regarding economic success, family background, test scores and years of schooling; describes the statistical methods; and pinpoints the reasons for differences between the nine principal samples. An appendix describing the samples used in this study is available as a separate document.) (EM)

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ED143854

THE EFFECTS OF FAMILY BACKGROUND, TEST SCORES, PERSONALITY TRAITS AND EDUCATION ON ECONOMIC SUCCESS

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April 1977

U.S. DEPARTMENT OF HEALTH,
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BIBLIOGRAPHIC DATA SHEET

1. Report No. DLMA-NIE-G-74-007-1

2.

3. Recipient's Accession No.

4. Title and Subtitle
Effects of Family Background, Test Scores, Personality Traits, and Schooling on Economic Success

5. Report Date
April 30, 1977

6.

7. Author(s) Christopher Jencks, Susan Bartlett, Mary Corcoran, James Crouse, David Eaglesfield, Gregory Jackson, Kent McClelland,

8. Performing Organization Rept. No.

9. Performing Organization Name and Address Peter Mueser, Michael Olneck, Joseph Schwartz, Sherry Ward, Jill Williams
Center for the Study of Public Policy
123 Mount Auburn Street
Cambridge, Massachusetts 02138

10. Project/Task/Work Unit No.

11. Contract/Grant No.
NIE-G-74-007

12. Sponsoring Organization Name and Address
U.S. Department of Labor
Manpower Administration
Office of Research and Development
601 D Street, N.W., Washington, D.C. 20213

13. Type of Report & Period Covered
Final

14.

15. Supplementary Notes

16. Abstracts The report analyzes ten surveys of men aged 25-64 to determine the effects of family background, adolescent personality traits, cognitive test scores, and years of schooling on occupational status and earnings in maturity. Data on brothers indicate that prior research has underestimated the effect of family background on earnings. The effect of background on occupational status and earnings is only partly explained by its effect on cognitive test scores or years of schooling. Adolescent test scores indicate that cognitive skills have a substantial effect on occupational status and earnings independent of background. These effects derive largely but not exclusively from the fact that test scores affect years of school. Data on adolescent behavior indicate that adolescent personality traits may exert as much impact on economic success as cognitive skills. Controlling background and adolescent test scores indicates that less than half the observed association between years of schooling and earnings is causal. Each extra year of schooling appears to raise earnings by 3-6 percent. Economic benefits of schooling fell between 1939 and 1949, but were stable from 1949 to 1969.

17. Key Words and Document Analysis. 17a. Descriptors

17. a) Aptitude Tests, Earnings, Education, Intelligence Tests, Motivation, Negroes, Personality, Personality Tests, Psychological Tests, Socioeconomic status, surveys, Tests

17b. Identifiers/Open-Ended Terms

17c. COSATI Field/Group 5C

18. Availability Statement Distribution is unlimited.
Available from National Technical Information Service, Springfield, Va. 22151.

19. Security Class (This Report)
UNCLASSIFIED

20. Security Class (This Page)
UNCLASSIFIED

21. No. of Pages

22. Price

THIS FORM MAY BE REPRODUCED

Preface

This report describes a two year study of the relationship between personal characteristics and economic success among 25-64 year old American males. It is a collaborative work, involving 12 different researchers. All the authors devoted considerable effort not only to the chapters on which their names appear but to chapters for which they receive no explicit credit. Christopher Jencks directed the project.

We have divided the report into three volumes. Volume I contains our substantive findings regarding the impact of family background, cognitive skills, personality traits, and years of schooling on occupational status and earnings. It also analyzes changes over time in effects of race, education, and labor force experience on income.

Volume II contains our methodological chapters. It begins with three chapters examining our measures of economic success, family background, test scores, and years of schooling. These chapters also describe our statistical methods. The next three chapters try to pinpoint the reasons for differences between our nine principal samples.

Volume III includes the ten appendices describing our ten samples in detail.

This document constitutes our final report to the National Institute of Education and to the Employment and Training Administration of the Department of Labor pursuant to Grant # NIE-G-74-0077. The research was conducted under the auspices of the Center for the Study

of Public Policy, with the collaboration of the MIT-Harvard Joint Center for Urban Studies and the Harvard Center for Educational Policy Research. The Joint Center's work in this area was supported by grants from the Department of Health, Education, and Welfare's Office of Income Security Policy Research and Evaluation. The Center for Educational Policy Research supported Olneck's study of brothers raised in Kalamazoo, Michigan, using funds provided by the Carnegie Corporation of New York, a doctoral fellowship from the Manpower and Training Administration, and a Ford Foundation grant to Samuel Bowles and Herbert Gintis. None of the funding agencies made any attempt to influence our work, and none is in any way responsible for our conclusions.

We are indebted to David Featherman and Robert Hauser for making available tabulations from their 1973 replication of the "Occupational Changes in a Generation" (OCG) survey and for a copy of the original OCG data tape. The Survey Research Center (SRC) at the University of Michigan made available data tapes from its "Productive Americans" and "Panel Study of Income Dynamics" surveys. William Mason kindly provided a copy of the 1964 veterans survey tape. The Project Talent Data Bank retrieved data on 11th graders at our request and allowed Marsha Brown to retrieve data on siblings. Michael Olneck generously shared his Kalamazoo data with the rest of us.

Jan Lennon administered the project, typed manuscript, and helped us all to maintain our sanity under a deluge of numbers that often threatened to overwhelm us. Irene Goodsell and Sara Hazel also typed much of the manuscript. None of them is responsible for the appearance of the final product, however, which reflects the fact that we ran out of money before completing our work.

Zvi Griliches, Andrew Kohen, and Paul Taubman made extremely helpful comments on an earlier draft of this report. Since we did not always heed their advice, they are obviously not responsible for any remaining errors.

Basic Books expects to publish a substantially revised and abbreviated version of this report in 1978.

Christopher Jencks
April 30, 1977

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Chapter 1

OBJECTIVES AND RESULTS OF THE RESEARCH

By Christopher Jencks

This report describes a three year investigation of the relationship between personal characteristics and economic success among American males aged 25-64. It focusses on those personal characteristics that are determined before a man enters the labor market: his family background, his cognitive skills and personality traits in adolescence, and the number of years of schooling he completes. It does not devote much attention to men's experiences after entering the labor market or to personal characteristics that depend on such experiences. Thus we do not anaylze the effects of work experience or on-the-job training in any detail. Nor do we look at physical or mental health, or at current region of residence (except insofar as this helps to explain the effects of region of birth). Our aim is not to provide a complete picture of the worker characteristics that employers value. Rather, it is to assess the degree to which birth, childhood, and adolescence determine adult success.

When we launched this project in 1973 we were concerned with three major deficiencies in earlier work (including our own) on these issues.

(1) Previous investigators had seldom had adequate measures of family background, cognitive skills, or personality traits for representative national samples.

(2) Previous investigators had often made statistical assumptions about the ways family background, cognitive skills,

and educational attainment affected success without testing these assumptions empirically.

(3) Previous investigators had often reached contradictory conclusions even when they asked apparently similar questions and used apparently similar data.

We therefore set out to assemble better samples, to analyze these samples more thoroughly, and to explain the discrepancies we found. We have not achieved any of these objectives fully. We have, however, made some progress in each area. The first part of this chapter describes how we pursued each objective. The second part summarizes our substantive findings. The last part suggests some policy implications of our work.

Samples

Our target population was restricted to men in the civilian labor force who were not in school full time and who had positive earnings during the year under study. Chapter 14 discusses the effects of these restrictions in detail. We used four large national samples of 25-64 year olds.

(1) The 1962 "Occupational Changes in a Generation" (OCG) sample collected by the US Current Population Survey (CPS). This sample has been extensively analyzed by Otis Dudley Duncan and his collaborators. The 1973 replication of this survey (OCG-II) was not available to us, but David Featherman and Robert Hauser generously provided certain key tabulations.

(2) The 1965 "Productive Americans" (PA) sample collected by the University of Michigan Survey Research Center (SRC).

(3) The 1968-75 "Panel Study of Income Dynamics" (PSID), also collected by SRC. This sample has been extensively analyzed by James Morgan and his collaborators.

(4) The Census Bureau's 1/1000 Public Use Sample from the 1970 Census. This sample has been extensively analyzed by economists too numerous to mention.

These four samples provide a baseline for assessing our six special purpose samples, which cover more restricted populations. These are:

(5) The 1973-74 NORC Brothers Sample. This survey was conducted at our request and has not previously been analyzed in any detail.

(6) The Census Bureau's 1966-71 National Longitudinal Survey of Older Men (NLS). Herbert Parnes of Ohio State University has been the principal investigator concerned with these data.

(7) The 1964 CPS Veterans sample. This sample is restricted to veterans under 35. It has been analyzed by Duncan (1968) and Griliches and Mason (1972).

(8) Project Talent's 1960-72 Longitudinal Sample. This subsample from the full Talent sample is restricted to students who were in 11th grade in 1960 and who were recontacted in 1972. It has not been previously analyzed.

(9) Project Talent's 1960-72 Brothers Sample. This subsample includes pairs of brothers enrolled in grades 11 and 12 in 1960 who returned a mailback questionnaire in 1971 or 1972.

(10) Michael Olneck's 1928-74 Kalamazoo Brothers Sample. This sample covers men who were 6th graders in Kalamazoo, Michigan, between 1928 and 1950, who had brothers in these same schools, and whom Olneck followed up in 1973-74.

Appendices A-J give detailed descriptions of each sample, including the relevant questions, coding, response rates, frequency distributions, means, standard deviations, breakdowns, correlations, and regressions. These appendices also explore statistical differences between samples. Table 1.1 summarizes some of their most salient characteristics. It should be obvious that no two samples are strictly comparable. Taken together, however, they provide a more complete picture of the relationship between youthful characteristics and adult success than has previously been available.

Table 1.1

Characteristics of Subsamples from Ten Surveys

Survey Name[1]	OCG	PA	PSID	Census	NORC **Brothers**
Survey Organization[1]	CPS	SRC	SRC	Census	NORC
Year of Initial Survey	1962	**1965**	1968	1970	1973-74
Initial Age	25-64	25-64	22-61	25-64	25-64
Year of Followup	--	--	1972	--	--
Age at Followup	--	--	26-65	--	--
Percent With No Data[2]	17	16	38	3+	NA[3]
Percent With Partial Data[5]	20	15	25	30	45[6]
N With Complete Data	11509	1188	1776	25697	150[7]
Sample Restrictions					
Positive Earnings	YES[8]	YES	YES	YES	YES
Non-Military	YES[8]	YES	YES	YES	**NO**
Household Head	NO	YES	YES	NO	NO
Non-Student	NO	YES	YES	YES	YES
Had a Brother	NO	NO	NO	NO	YES
Test Score Floor	NO	NO	NO	NO	NO
Education Floor	NO	NO	NO	NO	NO
Variables Measured[9]					
Race	D	D	D	D	D
Region of **Upbringing**	D	D	D	D	--
Father's Education	G	G	G	--	D
Father's Occupation	D	--	G	--	D
Number of Siblings	D	G	G	--	D
Father Absent at 16	D	--	--	--	D
Adolescent Personality	--	--	--	--	--
Adolescent Test Score	--	--	--	--	--
Early Adult Test Score	--	--	--	--	--
Adult Test Score	--	--	G	--	--
Years of Education	G	G	G	D	D
Degrees	--	**G**	**G**	--	--
Occupation	D	G	G	D	D
Earnings	G	D	D	D	G
Weeks Worked	--	G	D	G	--
Brother's Education	G	--	G	--	D
Brother's Occupation	--	--	--	--	D
Brother's Earnings	--	--	--	--	G

Survey Name[1]	NLS	Veterans	Talent	Talent Brothers	Kalamazoo
Survey Organization[1]	Census	CPS	Talent	Talent	Olneck
Year of Initial Survey	1966	1964	1960	1960	1928-50
Initial Age	45-59	25-34	16±	16-17	11±
Year of Followup	1971	--	1972	1971-72	1973-74
Age at Followup	50-64	--	28±	28±	35-54
Percent With No Data[2]	16	11	12	72[4]	55[4]
Percent With Partial Data[5]	40	30	38	65[6]	44[6]
N With Complete Data	2580	1438	839	99[7]	396[7]
Sample Restrictions					
Positive Earnings	YES	YES	YES	YES	YES
Non-Military	YES	YES	YES	YES	YES
Household Head	NO	NO	NO	NO	NO
Non-Student	YES	YES	YES	YES	YES
Had a Brother	NO	NO	NO	YES	YES
Test Score Floor	NO	YES	NO	NO	NO
Education Floor	NO	NO	YES	YES	YES
Variables Measured					
Race	D	D	D	D	NV
Region of Upbringing	D	D	D	D	NV
Father's Education	G	G	G	G	G
Father's Occupation	D	D	G	G	D
Number of Siblings	--	--	D	D	D
Father Absent at 16	D	D	D	D	D
Adolescent Personality	--	--	D	D	G
Adolescent Test Score	--	--	D	D	D
Early Adult Test Score	--	G	--	--	--
Adult Test Score	--	--	--	--	--
Years of Education	D	G	G	G	G
Degrees	--	--	D	D	--
Occupation	D	D	G	G	D
Earnings	D	G	D[10]	D[10]	G
Weeks Worked	D	--	--	--	--
Brother's Education	--	--	--	G	G
Brother's Occupation	--	--	--	G	D
Brother's Earnings	--	--	--	D[10]	G

Notes for Table 1.1

1. Abbreviations for organizations and surveys are defined in the text.

2. This is the ratio of "non-respondents" to "potential respondents." A "non-respondent" is any individual who could not be located or refused the interview at either the initial interview or the followup. Note that this non-response rate is for the entire target population of the original survey, not for our target population of men 25-64 who were not in school, military service, or institutions.

3. NORC used a block quota sample, so the non-response rate is indeterminate. NORC officials believe that block quota samples yield results similar to full probability samples, but they have not published evidence on this issue.

4. This is a non-response rate for individuals, not pairs. The exact non-response for individual Talent brothers is unknown, because we only retrieved individuals with a sibling who had returned followup data. The estimate in the Table is the individual response rate for the entire 11th grade sample in the 1972 followup. The estimated non-response for Talent brothers is higher than for the "representative" Talent sample because Talent had made no special effort to locate individuals in our subsample of brothers who had failed to return the mailback followup, whereas it did make such as effort for our "representative" sample.

5. This is the ratio of respondents in our target population with incomplete data on one or more of the variables that interested us to all respondents in our target population.

6. Item non-response for pairs of brothers includes failure to provide sufficient information on one's brother for the survey organization to locate the brother. It also includes the second brother's refusal to be interviewed. While Talent made no special effort to locate brothers, men who returned the mailback followup were likely to have a brother who did so too.

7. Number of pairs with complete data.

8. Our OCG tape had income but not earnings. It grouped men with negative incomes and men with $1-499 together. We eliminated men with zero income. The complete data sample also eliminates men without occupations. This presumably eliminates virtually all zero earners. It retains self-employed men who lost money. Due to an error, we did not eliminate military personnel from the basic OCG sample, but we did eliminate them from the complete data sample.

9. Variables recorded in detailed categories are denoted by a "D." Variables recorded in grouped form are denoted by a "G." Variables not measured are denoted by a dash (--). Variables with no variance are denoted NV. For a description of "detailed" and "grouped" categories, see the appendices.

10. Talent allowed respondents to report earnings on an hourly, weekly, or annual basis. We reduced all these reports to an hourly basis.

Measures of Economic Success

Like virtually all previous investigators, we measured economic success in terms of occupational status and earnings. Unlike most previous investigators, we have not concentrated on one of these measures to the exclusion of the other. Instead, we looked at both and tried to contrast results obtained using occupational status to results obtained using earnings.

We measured occupational status using Duncan's (1961) socio-economic index. This index runs from 3 to 96. It is based on the educational requirements and economic rewards of an occupation. To calculate a man's score, Duncan first assigned him to one of the Census Bureau's detailed occupational categories. He then assigned each of these categories a status score, based on the percentage of men working in the occupation in 1950 who had completed high school and the percentage with incomes of $3,500 or more. Since an occupation's rank depends on its educational requirements, education inevitably influences a man's occupational status. This is not just a methodological artifact. Rather, it reflects a real social phenomenon: the average education of men in a given line of work is closely related to the cognitive complexity and desirability of the work. It affects the social prestige of those who engage in the work, as well as their children's life chances, independent of the occupation's economic rewards. Chapter 11 discusses this index in more detail.

Except in Project Talent, we measured earnings on an annual rather than a weekly or hourly basis. This was partly because we believed that most of our surveys had collected more accurate data on annual earnings than on weekly or hourly earnings. In addition, we assumed that annual earnings provided a better measure of economic well-being than weekly or hourly earnings, since we assumed that variations in weeks and hours worked were primarily involuntary. Because there has been considerable controversy about the best way to scale earnings, we tried three different procedures. First, we looked at actual earnings, measured in dollars.

This yields estimated effects in dollars. These coefficients are easy to interpret, but their expected value changes from year to year with inflation and overall increases in productivity, so it is hard to compare one survey to another or to generalize past results to present circumstances. Second, we looked at the natural logarithm of earnings (Ln Earnings). This yields the percentage effect on earnings of a unit change in a given trait. Such coefficients are unaffected by inflation or general increases in productivity. Finally, we looked at the determinants of the cube root of earnings ($\text{Earnings}^{1/3}$). For reasons discussed in Chapter 11, we believe that subjective well being ("utility") is more nearly a linear function of $\text{Earnings}^{1/3}$ than either Earnings or Ln Earnings.

These three alternative measures of earnings yield essentially similar results. Under these circumstances Ln Earnings has two advantages over Earnings and $\text{Earnings}^{1/3}$. First, Ln Earnings is especially sensitive to variations near the bottom of the earnings distribution. This coincides with the emphasis of public policy over the past decade, which has focussed on altering the bottom of the earnings distribution rather than the top. Second, Ln Earnings yields coefficients that are easier to compare across time. Most of our analyses therefore concentrate Ln Earnings. Chapter 11 discusses these measures in more detail.

We concentrated exclusively on men who actually worked for pay. We excluded men outside the labor force, as well as a handful of men who were in the labor force but unable to find any paid work whatever during the year preceding a given survey. In principle, we would like to have included these individuals , but there is no generally agreed upon basis for measuring their economic success. Our decision to ignore non-participants makes our equations for Ln Earnings drastically different from those of economists who assign all non-participants an arbitrary value like $1.00.

Assigning non-participants $1.00 is virtually equivalent to analyzing the determinants of labor force participation instead of the determinants of relative earnings among labor force participants. Chapters 14 and 16 discuss this issue in more detail.

Measures of Worker Characteristics Prior to Entering the Labor Market

Family Background. We define family background as everything that makes men born into one family different from men born into another family. Most previous investigators have measured family background in terms of what we will call "demographic" advantages. By this we mean such readily measurable background characteristics as race, place of birth, father's education, father's occupation, number of siblings, and whether the respondent lived with both parents while he was growing up.[1] One can obviously augment this list by including mother's education, mother's occupation, parental income, parental ethnicity, parental religion, and the like. But no such list is ever complete. Thus while analyses of this kind can set a lower limit on the overall impact of family background, they can never set an upper limit. To get around this difficulty we have used an alternative approach developed by psychologists half a century ago. We look at the degree of resemblance between brothers. Such resemblance can be due to common genes, common environment, or the influence of one brother on the other. Unless brothers deliberately become unlike one another, however, resemblance between siblings sets an upper limit on the explanatory power of their common environment and genes. Unfortunately, since brothers share half their genes, resemblance between siblings does not allow us to estimate the effects of common environment alone. Nor does it allow us to estimate the effects of genes alone.

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[1] These are the principal background measures available from the Current Population Survey's 1962 "Occupational Changes in a Generation" (OCG) sample, which provided the basis for Blau and Duncan's classic study The American Occupational Structure (1967).

Table 1 lists our three samples of brothers. All three samples are small. Corcoran and Jencks analyze all these data in Chapters 2 and 3. [2]

Cognitive skills. Previous investigations of the effects of cognitive skills on economic success have almost all relied on a single cognitive test, usually designed to measure academic aptitude or intelligence.[3] Project Talent, in contrast, administered more than 50 cognitive tests to a national sample of high school students in 1960. Talent recontacted a representative subsample of former 11th graders in 1972, when they were about 28, and obtained data on their education, occupational status, and earnings. The Talent data therefore allow us to explore the effects of different adolescent cognitive skills in far greater detail than previous investigators.

With the exception of Taubman and Wales (1974), most previous investigations of the relationship between adolescent cognitive skills and later economic success have measured success when workers were still quite young. Talent is no exception in this regard. But Olneck's Kalamazoo sample is 35 to 59. Since Olneck's data cover brothers, they also allow us to distinguish the effects of cognitive skills from the effects of family background more adequately than previous investigators. Crouse analyzes the effects of cognitive skills in Chapter 4.

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[2] For other analyses of sibling data see Taubman (1976), Brittain (1977), Chamberlain and Griliches (1973), Jencks et al. (1972), and Blau and Duncan (1967).

[3] But see Taubman and Wales (1974) and Griliches (1976).

Personality traits. Most previous research on the effects of personality traits has relied on cross-sectional data. This makes it very difficult to say whether "favorable" personality traits cause economic success or vice versa. The Talent and Kalamazoo surveys probably provide the best longitudinal data on personality traits now available. The Kalamazoo schools collected teacher ratings of 10th graders' personality traits. Project Talent collected a wide range of self-assessments from its high school respondents. It also asked respondents to describe their high school behavior. Variations in such behavior presumably reflect personality differences to some extent. Mueser analyzes these data in Chapter 5.

Education. We measured education in the same/way as most previous researchers, looking at the highest grade of school or college the respondent had completed. But because we had data on brothers, we have been able to disentangle the effects of education from the effects of unmeasured parental characteristics. Our work is parallel in this respect to that of Chamberlain and Griliches (1973) and Taubman (1976). Furthermore, because we had test score and personality data collected prior to school completion, we have been able to estimate of the extent to which schooling is a proxy for these causally prior traits. Olneck discusses these data in Chapter 6.

Statistical Methods

Like most sociologists who have studied the determinants of individual economic success, we will use what has come to be known as a "life cycle" model. Such models divide an individual's life into a series of discrete stages. They then measure the extent to which individuals who are advantaged in one or more respects at one stage of life continue to be advantaged in these or other respects at subsequent stages. We assume that a worker's characteristics at an early stage of life can influence his characteristics at later stages, but that the reverse is impossible. We divide life into six stages: conception, childhood (birth to about the age of 10), adolescence (age 11 to school completion), early adulthood (school completion to age 24), maturity (age 25 to age 64), and old age (65 and over). We will not have much to say about advantages at conception. Such advantages are exclusively genetic, and our surveys provide no measures of genotype. Thus while we will occasionally take note of the fact that our results might look different if we could control genetic advantages, we will not try to quantify either the absolute effect of genes on economic success or the extent to which ignoring genes has biased our results. We also ignore old age, partly for lack of suitable data and partly because far fewer men are economically active after the age of 65. This means we will focus on the four "middle" stages: childhood, adolescence, early adulthood, and maturity.

In considering the effects of any given worker characteristic, we will ask three questions:

(a) How strong is the observed relationship between a worker characteristic and his economic success? Note that we will not be satisfied with merely establishing the existence of a relationship. Rather we will try to determine the size of the relationship. The size of a given relationship depends on the population one studies, the way in which one asks and codes questions, and the statistics one uses to describe the results. This means we will have to devote considerable attention to technical details in order to make our statements about the size of relationships at all meaningful. We will describe observed relationships using two kinds of statistics: correlations and regression coefficients. In this context readers should think of correlations as standardized regression coefficients. We use them when discussing relationships that have no "natural" metric.

(b) How much of a trait's observed relationship to economic success is a spurious byproduct of the fact that both the trait in question and economic success depend on prior traits? If we want to assess the true effect on economic success of staying in school rather than dropping out, for example, we must compare groups of respondents who had the same characteristics in adolescence but who then got different amounts of schooling. To accomplish this we will use multiple regression equations. To assess the effect of schooling on occupational status, for example, we regress occupational status on schooling while controlling all worker characteristics that are causally (i.e. temporally) prior to leaving school. These traits include family background, adolescent test scores, and adolescent personality traits.

(c) What are the mechanisms by which a given characteristic exerts its influence on economic success? To answer this question we augment our regression equation by including worker characteristics that are causally subsequent to the characteristic under study. Thus, if we want to say how education affects economic success, we may control test scores in maturity, years of labor force experience, and other traits that could depend on education. As we add each of these "intervening" variables to our regression equation, the coefficient of education will change. If we could identify all the relevant intervening variables and could measure them correctly, the coefficient of education might fall to zero. If we cannot identify (or properly measure) all the relevant intervening variables, the coefficient of education will remain positive. The ratio of the coefficient after controlling an intervening variable to the coefficient with only causally prior variables controlled tells us how much of the effect of education works through this intervening variable.

According to sociological convention, the effects of one variable on another that are not explained by intervening variables are known as "direct" effects, while the explained effects are known as "indirect." The magnitude of the "direct" effects is thus a function of the investigator's choice of "intervening" variables. In most cases one can, in principle, keep specifying new intervening variables and thus explain the entire effect of one variable on an other. Thus what looks like a direct effect to one investigator may look to another investigator like an effect requiring further research to specify the intervening variables that "explain" it. The claim that certain intervening variables are logically "appropriate" while others are logically "inappropriate" depends on theory, not data.

Unlike many previous analysts, we have not assumed that the effects of worker characteristics on economic success are either linear or additive. Chapter 12 describes our methods in some detail. In brief, we tested for nonlinearity in three ways.

1) When we estimated the effects of demographic background, test scores, and personality traits, we tested the significance of squared variables. We usually subtracted a constant from each variable before squaring it, so as to make the squared variable uncorrelated with the original variable. Adding the squared term to a regression equation then left the original linear coefficient essentially unchanged. This kept the linear coefficients comparable across samples, regardless of whether the squared term was significant, and allowed us to drop squared terms when they were insignificant. We recommend this procedure to other investigators concerned with comparability. We found a number of small but consistent non-linear effects.

2) In testing for the non-linear effects of education, we used a somewhat different procedure. We first estimated the effect of the average year of elementary or secondary education. Then we used a spline variable to estimate the difference between the effect of the average year of elementary or secondary education and the effect of the average year of higher education. Finally, we used a dummy variable to estimate the value of college graduation, over and above the linear effects of 12 years of elementary and secondary education and 4 years of higher education. Our tables denote these three measures as Years of Education, Years of Higher Education, and College Graduation. Most analyses include all three, even when the deviations from linearity (i.e. the coefficients of Years of Higher Education and College Graduation) are not significant. The non-linear effects of education are important for occupational status but not for Ln Earnings.

3) We looked for "bumpy" non-linearities by calculating the mean and standard deviation of education, occupational status, and earnings for discrete values of father's education, father's occupation, siblings, age, test scores, and education. We compared the value of eta^2 from these analyses of variance to the correlations obtained from linear and non-linear regressions. These tables appear in the appendices. Eta^2 is generally close to R^2.

We also tested for interactions in two ways.

1) We calculated the products of the more important worker characteristics. We retained these multiplicative variables when they significantly improved our ability to predict economic success in a given sample. In order to keep the linear coefficients comparable across samples, we usually made the multiplicative interaction terms uncorrelated with their components. No multiplicative interaction was consistently significant in different samples. Even the signs were inconsistent.

2) We also ran separate regressions for whites and non-whites, for men with white-collar, blue-collar, and farm fathers, for men born in different decades, and for men with high, medium, and low test scores. Schwartz and Williams compare the white and non-white regressions in Chapter 8. The other regressions appear in the appendices. There are no consistent differences.

Findings about the Effects of Specific Traits

Table 1.2 summarizes our findings regarding the effects of family background, adolescent test scores, adolescent personality traits, and years of education. For simplicity, Table 1.2 treats each cluster of characteristics as if it were a single composite variable. Each composite variable should be thought of as the weighted sum of several measures. "Demographic Background", for example, is the weighted sum of father's education, mother's education, father's occupation, number of siblings, parental income, and dichotomous variables for having a white collar father, having a father who was living at home when you were 16, growing up in the North, not growing up on a farm, and having a white skin. "Overall Family Background" is the weighted sum of all characteristics that make brothers alike. The "Personality Index" is the weighted sum of 108 self-assessments plus responses to more than fifty questions about one's behavior in high school. Wherever possible we adjusted the weights so as to maximize the correlation between each index and each measure of economic success. Since the indices have no "natural" metric, Table 1.2 standardizes all the coefficients. I estimated these coefficients from data presented in chapters 2 through 6 and in the appendices. These data are filled with puzzles and contradictions. I have chosen what I regarded as the most plausible estimates for our target population of male workers aged 25 to 64, but this often required extrapolation from more restricted samples and averaging discrepant results. The reader should therefore treat all the coefficients in Table 1.2 as if they had standard errors of at least 0.05. After examining the evidence in subsequent chapters, skeptics may want to allow for even larger margins of error.

Table 1.2

Approximate Standardized Regression Coefficients of Worker Characteristics When Predicting Economic Success

	Occupational Status		Ln Earnings	
	NEW[1]	OLD[2]	NEW[1]	OLD[2]
DEMOGRAPHIC BACKGROUND INDEX				
No Controls:				
Uncorrected	.50	.39	.37	.25
Corrected for Error	.57	.45	.44	.29
Test Scores and Education Controlled:				
Uncorrected	.15	.12	.10	.10
OVERALL FAMILY BACKGROUND INDEX				
No Controls:				
Uncorrected	.61	.54	.50	NA
Corrected for Error	.66	.56	.53	.39
Test Scores and Education Controlled:				
Uncorrected	.30[3]	.12	.30[3]	.10
ADOLESCENT TEST SCORE				
No Controls:				
Uncorrected	.45	.38	.35	.24
Corrected for Error	.50	.44	.41	.31
All Background Controlled:				
Uncorrected	.33	.29[4]	.30	.10[4]
All Background + Education Controlled:				
Uncorrected	.17	.06	.18	.11
ADOLESCENT PERSONALITY INDEX				
No Controls: Uncorrected	.45	NA	.30	NA
Background and Test Scores Controls:				
Uncorrected	.31	NA	.27	NA
Background Test Scores, Education Controls:				
Uncorrected	.19	NA	.25	NA
YEARS OF EDUCATION				
No Controls:				
Uncorrected	.61	.61	.42	.33
Corrected for Non-linearity	.65	NA	.43	NA
Corrected for Error and Non-linearity	.74	.65	.48	.35
All Background + Adolescent Test Scores + Personality Controlled:				
Uncorrected	.50	.49	.20	.18

Table 1.2 footnotes

1/ Estimated from data in this report.

2/ Estimated from Jencks et al (1972), Appendix B, except for the income value in line 5, which comes from p. 239, fn. 36. Values for adolescent test scores are from Jencks et al's Figures B-1 and B-2, not from their Table B-1 and B-2. Items marked NA were not included in Jencks et al's analyses.

3/ The standard error of this estimate is closer to 0.10 than 0.05.

4/ This is Jencks et al's maximum estimate.

In order to see how these estimates alter or reinforce the picture created by earlier research, Table 1.2 also presents Jencks et al's (1972) estimates. Our results appear under the heading New, while the 1972 results appear under the heading Old.

Family Background (Chapter 3). Our data suggest that a one standard deviation difference in demographic background is associated with a difference of 0.50 standard deviations in current occupational status. The impact of any demographic background index depends, however, on how many demographic advantages it subsumes. Jencks et al used only father's education and father's occupation to predict economic success. They also restricted their sample to whites who were not born on farms. These restrictions meant that a one standard deviation difference in demographic background had less effect in their analyses than in ours. Demographic background has less effect on relative earnings (r=0.37) than on occupational status (r=0.50), perhaps because earnings fluctuate more from year to year than occupational status does.[4] About 70 percent of the effect of demographic background on both occupational status and earnings arises because background affects adolescent tests scores and educational attainment. Correcting for measurement error does not greatly alter any of these conclusions. The effects of demographic advantages appear to have fallen between 1962 and 1973.

When we turn to the overall effects of family background we must be more cautious, since our estimates depend on relatively small samples of brothers and on the assumption that brothers do not appreciably affect one

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4. Since the standard deviation of Ln Earnings is about 0.75, an advantage of 0.39 standard deviations is equivalent to about $e^{(.39)(.75)} -1 = 40$ percent. Chapter 11 discusses this logic in more detail.

another. Our data suggest that if our index of background advantages included all the unmeasured background characteristics that brothers have in common, its correlation with economic success would exceed the correlation of our demographic index with economic success by about 0.12. The unmeasured components of this index differ from the measured components in that they do not exert their influence primarily by affecting test scores and education. Furthermore, Chapters 2 and 3 show that the background characteristics that affect economic success with test scores and education controlled are almost unrelated to demographic advantages such as father's occupational status. We have no good way of saying what these unmeasured background characteristics are, or how they exert their influence. We can say, however, that Jencks et al seriously underestimated their importance.[5]

Cognitive Skills (Chapter 4) Our data suggest that adolescents whose test scores differ by one standard deviation (15 points in the conventional IQ metric) typically differ by about 0.45 standard deviations on measured adult occupational status and 0.35 standard deviations on measured earnings. These differences would be slightly larger if our measures were free from error. About a quarter of the observed relationship between adolescent test scores and adult occupational status and a seventh of the relationship between test scores and earnings is a byproduct of the fact that both test scores and status depend on family background. The rest of the observed relationship appears to be genuinely causal. About half the effect of test scores on both occupational status and earnings seems to arise because cognitive skills affect educational attainment. The other half arises because men with high scores enter higher status occupations and earn more money than men with the same amount of education but lower scores.

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[5] Estimates of the overall effect of family background on occupational status and earnings in Table 1.2 are based on Jencks el al's estimates of the correlations between brothers for these two outcomes. The estimates of overall effects with test score and education controlled are based on Jencks et al's path models. Jencks et al's path model treated father's occupation as an adequate proxy for all the factors that made brothers alike on occupational status and earnings. Jencks et al's data on occupational resemblance between brothers supported this assumption. Our data do not. Jencks et al had no data on earnings resemblance between brothers. They erroneously assumed that the only important missing background measure was parental assets.

Jencks et al did not have much data on the relationship between adolescent test scores and later success. They therefore concentrated on AFQT scores obtained after school completion. They assumed that AFQT scores depended partly on how much schooling men had had, and that an AFQT score would predict economic success better than a score obtained prior to school completion. In fact, however, adolescent scores exhibit almost the same relationship to adult success as AFQT scores obtained from older men. We have no explanations for this. It means, however, that Jencks et al underestimated the role of adolescent test scores, even though they did not underestimate the eventual role of adult scores so far as we can tell.

Personality Traits (Chapter 5) No single personality measure predicts economic success as accurately as a cognitive test does, but the combined effects of a number of different adolescent personality measures are as strong as the combined effects of different adolescent cognitive tests. Personality traits predict occupational status partly because they are proxies for family background, and partly because they are proxies for test performance, but they have substantial effects (beta=0.33) independent of background and test scores. Less than half their effect on occupational status depends on affecting educational attainment. Personality traits affect earnings in ways that are largely independent of background, test scores, and educational attainment. The effects of personality on earnings may be substantially larger than Table 1.2 implies, since the data come from men who are only 28. The best predictor of economic success appears to be what Talent respondents labelled "leadership" and Kalamazoo teachers called "executive ability."

Education (Chapters 6-8) Education is the best single predictor of both occupational status and earnings. It predicts occupational status far more accurately than earnings, partly because occupations that require a lot of education acquire status as a result. A year of higher education exerts far more impact on occupational status than a year of elementary or secondary education. If we scale education to allow for this, and if we eliminate measurement errors, men who differ by one standard deviation difference on educational attainment will typically differ by about 0.74 standard deviations on occupational status and 0.48 standard deviations on earnings. These differences are larger than those found by Jencks et al for three reasons. First, Jencks et al looked only at white non-farm men, and the effects of education are somewhat reduced by this restriction. Second, Jencks et al failed to allow for the non-linear effects of education on occupational status--the only non-linearity of much importance in our data. Third, Jencks et al used Siegel and Hodge's (1968) data on the accuracy of educational reports, and this estimate was too high.

In order to estimate the effect of education on economic success we must control family background and test scores. Olneck shows in Chapter 6 that these controls reduce the coefficient of education by about a quarter in the occupation equations and by about half in the earnings equations. If we convert to undstandardized coefficients, an extra year of schooling seems to raise earnings by 3-5 percent. This estimate fluctuates appreciably from sample to sample, however. Controlling adolescent personality traits does not appreciably alter this picture, since the personality traits that affect earnings do not seem to be strongly correlated with educational attainment.

Bartlett shows in Chapter 7 that the benefits of an extra year of education fell sharply between 1939 and 1949, apparently because the overall dispersion of earnings became more equal. The benefits of a year of education remained essentially stable from 1949 to 1969, as did the overall dispersion of earnings. The effects of higher education on earnings have fallen appreciably since 1969, especially for younger workers, but the effects of secondary education have remained stable. Unlike some previous researchers, Schwartz and Williams found that an extra year of education raised non-white earnings by about the same percentage as white earnings. The dollar value of education was thus higher for whites.

Explaining Inequality in Occupational Status. Hauser and Featherman (1976) show that the mean occupational status of men 25-64 rose from 39 points in 1962 to 43 points in 1973. The standard deviation of 25-64 year olds' Duncan scores only rose from 24.44 in 1962 to 25.22 in 1973. Since Duncan scores do not constitute a ratio scale, we cannot measure inequality in a formal way. Nonetheless, it seems safe to assume that occupational status was about as unequal in 1973 as in 1962. The standard deviation is restricted for men under 25, but it does not change in any consistent way from 25 to 64.

Relying largely on OCG, Jencks et al (1972) found that demographic background, test scores, and schooling explained about 39 percent of the variance in occupational status among white non-farm males aged 25-64. Our present analyses cover non-whites and men born on farms, and include these traits as independent variables. They also allow for the non-linear effects of education. As a result, demographic background and education can explain 45 percent of the variance in occupational status among OCG men 25-64. The figures for other samples are similar. Jencks et al's data suggested that unmeasured background characteristics explained little additional variance in occupational status. Our data on brothers suggest that overall family background explains an additional 5-7 percent of the variance in occupational status with education controlled. Since demographic background raises R^2 by 4 percent, unmeasured background must add 1 - 3 percent. Test scores raised R^2 by one or two percent in Jencks et al's data and in ours. Jencks et al had no data on personality traits. Mueser's Talent data suggest that such traits add about 0.02 to R^2 in equations that control background, test scores, and education. It is obviously risky to sum increases in R^2 from different samples, especially when the initial variances differ. Nonetheless, such summation can give us at least a crude estimate of the likely value of R^2 in a sample with comprehensive measures of background, test scores, personality traits, and educational attainment. Our data imply that R^2 could be as high as 0.45 + 0.02 + 0.02 + 0.02 = 0.51.

This estimate implies that if the men who change occupations from one year to the next were typical of the larger population the correlation between their successive occupational statuses would be at least 0.51. In fact, if we look at 25-64 year old men who reported to the Census that they had changed occupations between 1965 and 1970, the correlation between their statuses in 1965 and 1970 was 0.59. These men do not differ in any obviously relevant way from those who stay in the same occupation. Their status scores are slightly lower than average, both before and after changing occupations, but the variance in their statuses is almost the same as the variance for all 25-64 year olds. The correlation between education and occupational status is also virtually identical for changers and non-changers. This suggests that our measures of family background, adolescent test scores, adolescent personality traits, and years of schooling are explaining about 51/59 of the stable variance in occupational status among these men. It seems to follow that most of the unexplained variance in occupational status for the population as a whole is due to measurement error, factors that are unstable over time, and men's tendency to remain in whatever job they happen to enter, even when its status differs from what one would expect on the basis of their race, education, and so forth.

Explaining Inequality in Earnings. Schwartz and Williams show in chapter 8 that the real income of 25-64 year old men rose by an average of 2.8 percent per year between 1949 and 1973. This represents an increase of 0.03 to 0.04 standard deviations per year, compared to 0.013 standard deviations for occupational status. Schwartz and Williams also show that the standard deviation of income rose less rapidly than the mean during these years. The ratio of the standard deviation to the mean, often called the coefficient of variation (V), is a standard measure of inequality. It fell from 1949 to 1973. But the standard deviation of Ln Income (s_{ln}), another standard measure of inequality, rose during these same years. The reason for this apparent paradox is that V emphasizes the position of high earners relative to the mean, while s_{ln} emphasizes the position of low earners relative to the mean. What apparently happened during these years was that those in the middle of the distribution gained ground relative to those at both extremes.

Although there is some variation from one sample to another because of differences in survey design, measurement, and coding, both V and s_{ln} average about 0.75 for the men 25-64 who worked in 1970 (see Appendices A and D). The values are lower for young men and higher for older ones. If the distribution of Earnings were normal, V=0.75 would mean that those who ranked at the 16th percentile earned 25 percent of the mean while those who ranked at the 84th percentile earned 175 percent of the mean. If the distribution were log-normal, s_{ln} = 0.75 would mean that those at the 16th percentile earned 47 percent of the mean while those at the 84th percentile earned 212 percent of the mean. The actual distribution is closer to log-normal than normal. Among men 25-64, inequality in earnings increases quite steadily as men get older.

Public concern about inequality in men's earnings depends largely on the degree to which men's earnings determine their families' total income. Schwartz shows in Chapter 9 that a man's 1971 earnings correlated 0.92 with his family's total income from all sources in the PSID sample of 25-64 year old men. The standard deviation of these men's family incomes was almost identical to the standard deviation of their earnings. Since these men's total family income was higher than their earnings, their family income was more equally distributed than their earnings. This reflects three basic facts. First, while a wife's earning power (i.e., her potential hourly wage) is positively correlated with her husband's earning power, her propensity to work is negatively correlated with her husband's wage. As a result, wives' actual earnings end up virtually uncorrelated with their husbands' earnings. Inequality in the total earnings of husbands and wives is therefore less than the average level of inequality among husbands and wives separately. Second, transfer payments appreciably reduce inequality. Third, income from assets appreciably increases inequality. The first and second of these influences more than offset the third. Nonetheless, among families that include an adult male, the dispersion of family income today depends largely on the dispersion of male earnings.

Lifetime earnings are more equally distributed than annual earnings. Our data do not, however, tell us how equal lifetime earnings are. Survey researchers have always assumed that retrospective data on earnings were even less accurate than retrospective data on occupational status. As a result, none of our surveys asked respondents about their earnings at earlier stages of their lives. All we have are the longitudinal data collected by PSID and NLS. The PSID data now cover 8 earning years. The correlation between Ln Earnings in 1967 and 1968 is 0.886. This correlation falls as the interval lengthens, reaching 0.682 after 8 years. The correlation should eventually approach an asymptote roughly equal to the average percentage of variance explained by stable traits among men 25-64. As we shall see, this value is at least 0.35. But the asymptote could be as high as 0.68. If we averaged individuals' earnings over a 40 year interval, transitory influences (including measurement errors) would presumably average out close to the mean for all respondents. The effects of experience would average out completely. The variance of the means would thus be only slightly larger than the mean covariance between all pairs of years. Since experience explains two percent of the variance in our 8-year means, this covariance would average out to no more than 66 percent of the observed variance, and perhaps as little as 35 percent. Thus if the observed standard deviation of Ln Earnings averages 0.75 in a single year, the implied standard deviation over 40 years would be between $(0.75)\ (0.66)^{1/2} = 0.61$ and $(0.75)\ (0.35)^{1/2} = 0.44$. 6/

6/ This estimate assumes that measurement errors are uncorrelated with true values, and hence that the true variance of earnings is smaller than the observed variance. Matched CPS and Internal Revenue Service imply, however, that errors are negatively correlated with true values, and that the true variance is as large as the measured variance. If so, the true variance would be slightly larger than the figures in the text.

How much of the observed inequality in earnings can we explain in terms of men's characteristics before they enter the labor force? Our data suggest that educational attainment explains about 17 percent of the variance in Ln Income for OCG men 25-64. Demographic backgorund (primarily race and region of birth) raises the figure to 23 percent. Our results for earnings are quite similar. Samples of brothers suggest that if we had data on all aspects of background we could raise R^2 by 7 to 14 percent above the value obtained using education alone, with the higher value seeming the most plausible. This implies that unmeasured background adds 8 percent to R^2. Test scores never boost R^2 by more than 2.3 percent. Mueser's Talent data suggest that with demographic background, adolescent test scores, and education controlled, adolescent personality measures can raise R^2 by at least 6 percent. The increase might be even larger for an older sample.

Once again, it is hazardous but instructive to treat these increases in R^2 as if they were additive. If they were, we might expect to explain 23 + 2 + 8 + 6 = 39 percent of the variance in Ln Earnings using variables measured before a man enters the labor force. Labor force experience seems to explain another 3-4 percent of the variance in most samples. Errors in measuring earnings explains 7-11 percent. Errors in measuring workers' other characteristics explain some additional variance. This leaves about half the observed variance unexplained. The PSID data imply that at least 1-0.07-0.68=25 percent of the observed variance is attributable to transitory factors other than errors. This suggests that another 25 percent may be attributable to stable characteristics that are not included in our models. Alternatively, this 25 percent may also be attributable to transitory factors. Resolving this issue would require longer term longitudinal data.

Economists sometimes argue that unexplained variation in earnings is due to variations in working conditions. Other things being equal, a job that offers widely desired perquisites, such as short hours, physical comfort and safety, autonomy in how one does one's work, low risk of losing the job, or high prestige, will not have to pay as much to attract a given worker as a job that is short on these attributes. If such tradeoffs were widespread, they might help explain variation in earnings among men with similar backgrounds, cognitive skills, personality traits, and educational credentials. Unfortunately, we have no direct measures of workers' preferences on these matters, so we cannot say how much of the variation in earnings derives from variations in preferred working conditions. We know, for example, that a substantial fraction of the unexplained variance in annual earnings derives from variation in the number of weeks men work in a given year. In 1964 and 1969, when unemployment was relatively low among 25-64 year old males, Ln Weeks Worked raised R^2 by 0.13 in the PA and Census samples. In 1971, when unemployment was higher than in 1964 or 1969, Ln Weeks Worked raised R^2 by 0.20. But we do not know how much of the variation in weeks worked derived from variation in men's "taste for leisure". All we can say is that if variations in weeks worked reflect variations in the "taste for leisure," this taste cannot be very stable from one year to the next. In the PSID data, for example, the average correlation between weeks worked in sequential years from 1967 through 1974 was only about 0.2. This suggests that a stable "taste for leisure" cannot explain more than 20 percent of the variance in weeks worked. The remaining variance must be due to transitory factors, either "voluntary" or "involuntary."

At first glance we seem to have explained far more of the variation in annual earnings than Jencks et al explained using similar characteristics. Jencks et al presented data from OCG and from the Veterans survey suggesting that demographic background, test scores, and education explained only 14 percent of the osbserved variance in annual income within ten year cohorts of white non-farm men aged 25-54. They assumed that with test scores and education controlled, unmeasured background characteristics explained negligible amounts of additional variance. They attributed 10 percent of the unexplained variance to errors in measured income and one percent to errors in measuring demographic background, test scores, and education. This left 75 percent of the observed variance unexplained. The major sources of discrepancy between their results and ours are as follows.

(1) Jencks et al looked only at white non-farm males. When we broaden the sample to include non-whites and farm-born men, include these characteristics as independent variables, and add region of birth to the model, we explain 21 percent of the variance in annual income. But including non-whites and men born on farms also increases the total variance substantially. As a result, the absolute amount of unexplained variance in our samples is larger than in Jencks et al's samples (see chapter 14).

(2) Jencks et al averaged data for ten year cohorts of men rather than treating age or experience as an independent variable. We pooled all men 25-64, increasing the total variance, and then added experience as an independent variable. Once again this raises R^2. It does not appreciably reduce the absolute amount of unexplained variance.

(3) Jencks et al assumed that unmeasured background characteristics would explain negligible amounts of variance with education and test scores controlled. Our data suggest that unmeasured background could raise R^2 as much as 8 percent.

(4) Jencks et al had no data on personality traits. They suggested that such traits might account for an appreciable fraction of the unexplained variance. Our data suggest that at least 6 percent of the otherwise unexplained variance among young men is due to such traits.

Policy Implications

1. Equalizing opportunity.

"Equality of opportunity" is often defined as a situation in which children born into different families have equal chances of economic success. Our data suggest that family background affects economic success primarily but not exclusively by affecting educational attainment. This suggests that if we want to reduce the impact of family background on economic success, we could achieve a lot by reducing the impact of background on educational attainment. How this might be accomplished is, however, something of a mystery. The effects of family background on educational attainment have been quite stable throughout the twentieth century, despite massive changes in the character of the educational system (Hauser and Featherman, 1976).

Our data show that race, region of birth, and father's occupational status all affect economic success among men with equal amounts of schooling. But region of birth affects economic success only because it affects region of residence in maturity. Since regional differences in occupational status are negligible, and regional differences in earnings are both small and declining, there is no obvious reason for government concern with this issue. Race affects economic success even with schooling controlled. These effects also diminished between 1962 and 1973, presumably because of governmental intervention. Further reducing the effects of race will presumably require continuing government pressure. Father's occupation continues to affect the respondent's occupational status and earnings with education controlled, but the effects are small and do not reach statistical significance in most surveys. We do not know whether these effects are due to differences in the career choices of men from high and low status backgrounds, differences in job performance, or difference in perceived job performance (i.e. discrimination).

Thus it is hard to say whether governmental intervention would be appropriate.

Our data suggest that a number of other as yet unmeasured background characteristics also affect economic success independent of educational attainment. Since we cannot identify these influences, we cannot be sure whether they violate our norms regarding equal opportunity. If the unmeasured background influneces that make brothers alike are genetic in character, they may be consistent with our norms about equal opportunity. This is not necessarily true, however, since genes can affect economic success without affecting actual productivity (e.g. by affecting physical appearance). If the unmeasured background characteristics that affect economic success involve the home or community environment, they presumably violate our norms regarding equality of opportunity. Even so, I find it hard to imagine reducing the impact of home environment without drastically reducing the rôle of the family in socializing the young. Few advocates of equal opportunity are willing to pay this price.

2. Boosting Cognitive Skills.

Cognitive skills exert a substantial influence on economic success, in good part because they affect educational attainment. It may, however, be a mistake to infer that altering an individual's test performance will alter his subsequent economic success. Jencks and Brown (1975) found, for example, that changes in test performance between 9th and 12th grade did not affect subsequent educational attainment. This result may not be general, but it suggests that what affects educational attainment is not current cognitive skill but some stable underlying aptitude that affects both test performance in 9 th and 12 grade and education. If this holds for economic success as well, intervention programs that alter students' test scores may not alter their long term economic prospects.

3. Changing Personality Traits.

Adolescent personality traits appear to exert appreciable effects on subsequent economic success. Once again, however, it is not clear that changing adolescent behavior would alter individuals' subsequent success. Eleventh grade boys who had never gone steady, for example, earned more at 28 than 11th graders who had gone steady, even with everything else controlled. But it hardly follows that if parents prevented a boy from going steady this would raise his eventual earnings. Measures of this sort are likely to be proxies for other underlying traits, and changing the measured value is no guarantee that the underlying trait will change.

4. Staying in School.

Additional schooling has a substantial effect on an individual's eventual occupational status and a moderate effect on his earnings. Our best estimate is that each extra year of schooling raises earnings by 3-5 percent once all causally prior traits are controlled. Following Mincer's (1974) argument, the implied return to foregone earnings is also 3-5 percent. If the costs and benefits of schooling were exclusively monetary, this would make education a relatively poor investment for most individuals. But the costs and benefits of schooling are not exclusively monetary. Individuals attend school and college for a multitude of reasons, not the least of which is that they often enjoy it. They also expect a multitude of benefits, many of which are social and cultural rather than monetary. Thus while a 3-5 percent return is not high compared to many alternative forms of investment, especially given the risks involved, it makes good sense for students who also expect other benefits.

5. "Social" vs "Private" Returns to Human Capital.

Our quantitative data estimate the effect of changing an individual's characteristics on his status or earnings, i.e. "private" benefits of various traits. But public policy is usually more concerned with "social" than with "private" benefits. We want to know, for example, not how much another year of school will raise the student's own earnings but how much it will raise the national income. Suppose for simplicity that extra schooling has no non-monetary effects. If it also had no effect on national income, but merely allowed the individual who got it to displace some other individual from a lucrative job, the social benefits of education would be non-existent. Under these circumstances individuals might invest in education for selfish reasons, but society would have no reason to subsidize such investments. Indeed, it might want to tax them. If a year of schooling raised national income by precisely the same amount as individual earnings, the social and private benefits would be equal. The student would, however, be the sole beneficiary of his additional education. The rest of society would neither gain nor lose. The case for subsidizing such activities would remain weak, at least so long as education was evaluated in purely economic terms. Only if an extra year of education raised national income *more* than it raised individual earnings would the social benefit exceed the private benefit. Then subsidies would make sense in narrowly economic terms.

If the labor market were fully competitive, if information were all accurate and free, if workers could be hired and fired at no cost to their employers, if wages could be adjusted downward as well as upward, and if workers either accepted the legitimacy of paying everyone his marginal product or produced the same amount regardless of whether they felt their pay was fair, one might expect every worker to earn an amount equal to the market value of whatever he could produce. But these conditions are never met. Many workers are therefore paid less than they would be worth in a "perfect" market, while othere are paid more. In general, I would expect employers to over-reward workers with characteristics that conventional wisdom portrays as related to productivity: education, verbal ability, and white skin, for example. If that actually happens, the private benefit of changing such characteristics will exceed the social benefit. Giving everyone an extra year of school, for example, will raise national income by less than 3-5 percent. But our data provide no direct evidence on this point. Nor do they help us assess the non-monetary costs and benefits of education, which are probably more important than monetary ones.

6. Reducing Occupational Inequality.

Variation in men's characteristics when they enter the labor force explain about half the variance in men's subsequent occupational statuses. It does not follow, however, that if all men entered the labor force with identical characteristics the variance of occupational status would fall to half its present level. Suppose, for example, that everyone had the same amount of education. This would hardly push everyone into occupations that now have the same status. Society would still need unskilled manual laborers as well as highly skilled professionals, for example. Of course if everyone had the same amount of education, the relative rewards of different occupations might change substantially. Prospective manual workers would have more alternatives than they now do and would turn to other work unless unskilled manual work paid better than it now does. Professionals would, in turn, face more competition and would have a harder time earning premium wages than they now do. The economic distance between professional workers and manual workers would therefore diminish, and the Duncan Scale would have to be recalibrated. Changes in relative wages would, in turn, lead to some changes in the present occupational mix.

Our data tell us very little about the likely magnitude of such changes. Occupational differentials depend on many factors besides supply and demand, including government manipulation, managerial manipulation, and union manipulation designed to maintain past patterns in the name of fairness. Movement between occupations also depends on many non-monetary factors. All we can say with confidence is that one cannot treat our equations for predicting an individual's status under present labor market conditions as technological verities that would continue to hold true if the variance of worker characteristics changed. Thus while changing the variance of worker characteristics would change the variance of occupational status, we cannot predict the likely size of the change.

7. Reducing Inequality in Earnings.

Our data suggest that men's characteristics when they enter the labor market might explain 45 percent of the true variance in annual earnings. It is therefore tempting to infer that if all men entered the labor market with equally valuable cognitive skills, personality traits, and educational credentials, the variance of earnings would fall by 45 percent. If workers with different characteristics earn different amounts because they have different productive capacities, giving them the same (or at least equally valuable) traits should equalize their earnings.

In fact, however, workers' earnings also differ for many other reasons, some of which I mentioned in discussing social versus private returns to human capital. This has two implications. First, the variance of earnings is likely to exceed the variance in potential productivity - though it could, of course, be smaller. Second, worker characteristics that do not affect productivity may still have sizable effects on earnings. As a result, factors that "explain" variance in individual earnings may not "cause" this variance in any meaningful sense. Skin color is an obvious case in point. It does not seem likely that blacks earn less than whites solely because they cannot produce as much. It follows that making blacks indistinguishable from whites would not necessarily eliminate the variance in earnings now "explained" by race.

Our data therefore provide little guidance for those who would like to predict the effect on earnings of changing the distribution of personal characteristics. Our only really relevant evidence is Bartlett's analysis of changes in the dispersion of earnings between 1939 and 1969. She found a dramatic decline in earnings inequality between 1939 and 1949. There was no parallel decline in the variance of education or labor force experience. The variance of earnings appears to have declined for exogenous reasons, such as the reduction in unemployment. The decline in overall inequality, in turn, reduced the effects of both education and experience on earnings. Bartlett's data therefore suggest that overall wage inequality determines the value of education. They do not suggest that the degree of inequality in education affects the degree of inequality in earnings. While it would be a mistake to overgeneralize these results, they should warn us against assuming that changes in the distribution of personal characteristics are the sole, or even the prime, determinant of changes in the distribution of earnings.

Our data also imply that inequality diminishes if we look at earnings over a long interval. This does not, however, necessarily imply that public policy should concern itself only with long term inequality, ignoring short term fluctuations. Most men treat the person they will become in the future as something of a stranger, acting as if his welfare was far less important than their own. As a result, they seldom save or borrow as much as they would if they were trying to maximize their average well-being over a lifetime. Left to their own devices they will not insure themselves adequately against either unemployment or retirement. Public policy has long assumed that some degree of coercion is both necessary and desirable to deal with such "irrationality." Unemployment insurance and Social Security force us to even out the flow of income over our lives, even if we do not want to do so. Altering the distribution of personal characteristics would do little to solve this problem.

8. Further Research.

Most social scientists conclude reports like this by suggesting that if only they had had better data they could have drawn stronger conclusions. In this case, however, we would have needed completely different kinds of data to draw strong policy conclusions. Strong conclusions would require either natural or experimental changes in the governmental policies whose effects we want to predict. Since our surveys were not tied to such changes, they are of very limited value. Our data can tell us a fair amount about the likely effects on small numbers of individuals of changing the characteristics with which these individuals enter the labor market. They cannot tell us much about the likely effects of large scale changes, since these are likely to change the labor market itself.

Chapter 2

THE EFFECTS OF FAMILY BACKGROUND ON OCCUPATIONAL STATUS

By Mary Corcoran and Christopher Jencks

This chapter investigates the effects of family background on occupational status. We will define "family background" somewhat loosly as including all the consequences of having one set of parents rather than another. The term therefore subsumes not only the effects of parents per se, but the effects of the neighborhood and region in which the parents happen to live, the quality of the schools to which they send their children, and so forth.

Previous investigators (e. g., Blau and Duncan, 1967; Duncan, Featherman, and Duncan, 1972; Sewell and Hauser, 1975) have estimated the impact of what we will call "demographic" background characteristics, such as father's education, mother's education, father's occupation, region of birth, family size and race. These investigations have greatly expanded our understanding of determinants of a son's occupational status, but they have obvious limitations. One can never identify, much less measure, all the parental characteristics that could in principle affect a son's eventual occupational status. So long as some potentially relevant characteristics remain unmeasured, one will underestimate the explanatory power of family background by some unknown amount. Such analyses therefore set a lower limit on the explanatory power of family background but not an upper limit.

To get around this difficulty we will look at the degree of resemblance between brothers' occupational statuses. Resemblance between brothers sets an upper limit on the combined influence of all the measured and unmeasured background characteristics that brothers have in common. If brothers also influence one another, the intraclass correlation between brothers (r_I) will exceed the percentage of variance explained by measured and unmeasured family characteristics (R^2). If brothers do not influence one another, r_I will provide an unbiased estimate of this otherwise unobtainable R^2.

The remainder of this chapter has four sections. Section 1 uses our five large national surveys to estimate the effects of demographic background on a son's occupational status in maturity. Section 2 investigates the mechanisms by which demographic background characteristics affect a son's occupational status.

Section 3 uses data from our three surveys of brothers to estimate the importance of unmeasured background characteristics in determining occupational status. Section 4 explores the likely character of these unmeasured factors.

1. Effects of Demographic Background

The 13 "demographic" background characteristics that interest us are:

1. Race (White/Nonwhite)
2. Father's Birthplace (US/Other)
3. Father's Education (Highest Grade Completed)
4. Father's Occupation (Duncan Score)
5. Father White Collar (Yes/No)
6. Mother's Education (Highest Grade Completed)
7. Son's Region of Birth (South/Other)
8. Son Raised on a Farm (Yes/No)
9. Number of Siblings
10. Father Absent when Son was 15 (Yes/No)
11. Parental Income (1967 Dollars)
12. Ethnicity (Irish/Italian/Polish/French/German/Slavic/Spanish/British/Jewish/Black/Other)
13. Religion (Catholic/Jewish/Protestant)

These characteristics classify people according to principles that are understood by almost all members of society. This reflects the fact that they portray an individual's formal relationship to major institutions in American society: schools, employers, consumption, the family, and so forth. Since these institutions are pervasive, we have a shared vocabulary for discussing our relations to them. It is much harder to measure people's psychological characteristics, because there are no generally accepted external standards for making psychological judgments. As a result, there is a much better chance of getting the same answer from two brothers if you ask them the highest grade of school their father completed, or what he did for a living, or how many children he had, than if you ask whether he was "happy," "ambitious," or "intelligent."

We were tempted to call the 13 characteristics listed above "objective," implicitly contrasting them with "subjective" psychological measures on which we had no data. But other characteristics, like the number of hours the respondent spent with his mother each day and whether he was breast fed, are equally "objective." They are just harder to measure. The truth is that our list was not limited by the necessity of "objectivity" but by what was readily available. For lack of a better term, we eventually decided to label the available measures "demographic." This is not to say that demographers have been the only social scientists interested in the 13 characteristics listed above. That is by no means the case. But experience suggests that labelling these characteristics "demographic" conveys that we mean to most social scientists, even though it is not very helpful to laymen.

Not all our surveys provide information on all these items. Indeed, none of our surveys provides information on ethnicity, and only one (PSID) asked religion. Worse, not all our surveys measured these traits in the same way. Table 2.1 lists the items we used from each survey. The appendices give the precise coding of each item in each survey. Table 2.1 also lists two surveys for which we did not have the original data. These are the 1973 replication of OCG, from which David Featherman and Robert Hauser kindly authorized David Bills to make tabulations for us, and the 1964 follow-up of 1957 Wisconsin high school graduates, on which Sewell and Hauser (1975) provide data.

The top five rows of Table 2.1 describe five large national surveys of mature men. The standard deviation of Duncan scores is higher for the three surveys conducted by the Census Bureau (OCG, OCG-II, NLS) than for the two surveys conducted by the Survey Research Center (PA and PSID). The difference is due to the fact the SRC coded occupations in nine broad categories, whereas the Census Bureau used more than 400. The SRC coding eliminates the variance in occupational attainment that falls within its broad categories. Measured background characteristics explain 23-24 percent of the occupational variance in the three Census Bureau surveys, compared to 17 percent in the two SRC surveys. Again, the difference is largely due to the coding of the dependent variable, though differences in the independent variables also play a role.

None of the five large national surveys collected data on brothers. Only one (PSID) collected data on cognitive skills, and it did not use a very reliable test. The bottom rows of Table 2.1 therefore describe other samples that either covered brothers, administered reliable tests, or both.

Table 2.1: Relationships between occupational status (Duncan scores) and family background for civilian non-institutional non-student males with positive earnings and complete data.

Survey Organization (Year of Occupational Data)[a] (1)	Survey Name (2)	Additional Restrictions on Sample (3)	Age in Occupation Year (4)	Sample Size (5)	Background Measures[b] (6)	Test Score (7)	S.D. of Duncan Scores (8)	r_I for Siblings[c] (R^2) (9)
CPS (1962)	Occupational Changes in a Generation (OCG)	Includes students & men with earnings = 0 (if income ≠ 0)	25-64	11,504	1,3,4,5, 7,8,9,10	No	24.87	NA (.244)
SRC (1965)	Productive Americans (PA)	Heads of households	25-64	1,188	1,2,3, 7,8,9	No	20.61	NA (.175)
DSD (1967)	Parnes' National Longitudinal (NLS)	None	45-59	2,580	1,2,3,4, 5,7,8,10	No	24.79	NA (.240)
SRC (1972)	Panel Study of Income Dynamics (PSID)	Heads of households	25-64	1,774	1,2,3,4,5 7,8,9, 10	Yes	21.07	NA (.169)
CPS (1973)	Occupational Changes in a Generation (OCG-II)	Same as OCG	25-64	15,817	1,3,4,5,6, 7,8,9,10,11	No	25.40	NA (.226)
NORC (1974)	NORC Brothers	Brothers	25-64	300	1,3,4,5, 8,9,10	No	24.90	.371 (.189) d
CPS (1964)	Veterans	Veterans	30-34	803	1, 3, 4, 7,8,10	Yes	23.37	NA (.128)
Sewell et al. (1964)	Wisconsin	Non-farm men who reached 12th grade in Wisconsin	24	2,069	3,4,6,(7), (8),11	Yes	23.24	NA (.112)
Talent (1972)	Talent 28 Year Olds	Men who reached 11th grade	28(±1)	839	1,3,4, 9,10	Yes	23.74	NA (.112)
Talent (1971-2)	Talent Siblings	Brothers who reached 11th or 12th grade	28(±1)	198	1,3,4, 9 10	Yes	25.64	.321 (.141)
Olneck (1974)	Kalamazoo Brothers	Brothers who reached 6th grade in Kalamazoo, Michigan	35-59	692	(1),2,3,4, 5,6,(7), (8),9,10	Yes	23.16	.309 (.125)d

TABLE 2.1 NOTES

[a] CPS = Current Population Survey, U.S. Bureau of the Census
DSD = Demographic Surveys Division, U.S. Bureau of the Census
SRC = Survey Research Center, University of Michigan
Talent = Project Talent, American Institute for Research, Palo Alto
NORC = National Opinion Research Center, Chicago

[b] 1= white, 2= father born in U.S., 3= father's education, 4= father's occupation (Duncan scale), 5 = father white collar, 6 = mother's education, 7 = son's region of birth or upbringing, 8 = son raised on a farm, 9 = number of siblings, 10 = father absent when son 15 or 16, 11 = parental income at 17. Variables in parentheses have no variance due to sample restrictions.

c r_I= interclass correlation = $\bar{R}^2$ from a regression equation that includes a dummy variable for each family = R^2 adjusted for degrees of freedom. If all pairs are entered twice, with order reversed, as they are in our analyses, the resulting product-moment correlations will estimate the intraclass correlations.

OCG and OCG-II were as nearly comparable as the Census Bureau could make them, but the regressions described in Table 2.1 differ in several respects. The OCG regression includes non-linear terms and interactions, while the OCG-II regression omits them. Conversely, the OCG-II regression includes mother's education and parental income, which were not available in OCG. In order to investigate trends over time, we ran identical linear, additive regressions of occupational status on eight background measures from OCG and OCG-II. Table 2.2 summarizes the results. R^2 falls from 0.242 in 1962 to 0.208 in 1973, a modest but highly significant difference. The most conspicuous reasons for this change are that race, father absence, and region of birth have less effect on occupational status in 1973 than in 1962. Number of siblings has slightly more effect in 1973 than in 1962, but this difference might disappear if we knew the degree of non-linearity in OCG-II. The remaining differences are not statistically significant. These results suggest that we should assume a modest secular decline in the impact of demographic background on occupational attainment from 1962 to 1973.

Table 2.3 shows the regressions of occupational status on measured background characteristics. In the five national surveys, being white, having a father with lots of schooling, having a father in a high status or a white collar job, having relatively few siblings, being raised in the North rather than the South, not being raised on a farm, and not coming from a broken home all increase a man's expected Duncan score. The effect of coming from a broken home is reversed, however, once we control parental income (compare the OCG-II equations in Tables 2.2 and 2.3). This suggests that a father is valuable as a source of income, but not much else. Surprisingly, men with foreign-born parents are at no disadvantage relative to sons of native-born Americans who have otherwise similar backgrounds. Indeed, other things equal, men with foreign-born fathers are more likely to work in high status occupations

TABLE 2.2

Regressions of Occupational Status on Family Background Measures from OCG and OCG-II for men with complete data and non-zero incomes.

Sample		White	Father's Education	Father's Occupation	Siblings	Non-Farm Upbringing	Father White Collar	Father Absent	Non-South	SD of Residuals (R^2)
OCG	B	12.349	.807	.231	-.950	5.289	5.333	-4.216	1.335	21.668
	S.E.	(.730)	(.059)	(.017)	(.069)	(.528)	(.786)	(.566)	(.475)	(.242)
	r	.234	.321	.402	-.262	.209	.356	-.078	.160	
OCG-II	B	7.989	.868	.196	-1.247	4.595	4.113	[-1.037]	[-.037]	22.602
	S.E.	(.664)	(.053)	(.014)	(.073)	(.469)	(.672)	(.702)	(.409)	(.208)
	r	.174	.336	.385	-.278	.254	.332	-.017	.120	

[Coefficients in brackets are less than twice their standard error .]

Table 2.3

Sample and Control Variables

Background Measures	OCG		OCG-II	PA		PSID	
	None	Ed, Higher Ed, BA	None	None	Ed, Higher Ed, BA	None	Tests, Tests^2
White	12.6638	10.0313	7.2480	11,3758	8.515	8.6524	6.2908
Father's Education	.8346	.1044	.3584	1.4853	[.250]	1.1907	.9580
Father's Occupation	.2204	.1243	.1576			[.0228][b]	[.0155][b]
Father White Collar	4.9926	1.1579	3.8351			6.5966	5.4568
Father Native Born				[-.5635]	[-1.355]	-2.6819	[1.6848]
Father Absent	-4.4984	-1.3679	1.9562			[-4.7545]	[-3.7628]
# of Siblings	-.9574	-.2767	-1.0676	-.7404	[-.026]	-.8797	-.6052
Mother's Education			.6845				
Family Income			.00044[d/]				
Non-Farm	5.1189	3.8014	4.2441	7.4285	3.884	2.8314	[1.7899]
Non-South	1.2725	[-.4008]	[-.3139]	[-.4701]	[-1.308]	[.9860]	[.3923]
Father's Education^2	.0407	[.0066]		.0790	[-.025]		
Father's Occupation^2	-.0015	-.0020				-.0042	-.0047
Siblings^2	.0705	[.0064]				.1207	.1108
S.D. of Residual	21.630	18.500	22.350	18.771	15.732	19.262	18.606
R^2	.244	.448	.226	.175	.425	.169	.226

Table 2.3 (continued)

	PSID	NLS		Talent 28 year olds			
Background Measures	Tests, Tests^2, Ed, Higher Ed, BA	None	Ed, Higher Ed, BA, Vocational Trng.	None	Tests	Tests, Adolescent Aspirations	Tests, Higher Ed, BA
White	5.300	12.9083	8.6644	[2.4851]	[-3.4720]	[2.2574]	[2.9213]
Father's Education	[.2680]	1.1946	[.1580]	1.1252	.7090	[.3800]	[.1766]
Father's Occupation	[.0289]^b	.1817	[.0499]	.1724	.0816	[.0560]	[.0204]
Father White Collar	[-.3552]	[3.0628]	[3.3051]				
Father Native Born	[.7599]	[1.6769]	[1.7559]				
Father Absent	[.7419]	-3.9791	[.6109]	-5.7152	[-1.3935]	[.2613]	[.1866]
# of Siblings	[.0042]			-1.1390	[-.3908]	[-.2537]	[.0083]
Mother's Education							
Family Income							
Non-Farm	1.8817	10.4574	6.5376				
Non-South	[-.0430]	[1.0940]	[.0268]				
Father's Education^2		.0436	[.0327]				
Father's Occupation^2	-.0036						
Siblings^2	[.0061]						
S.D. of Residual	15.591	21.662	18.288	22.444	20.643	19.798	17.908
R^2	.458	.240	.459	.112	.250	.316	.437

Table 2.3 (continued)

	Veterans 30-34			Wisconsin 24 year olds			Kalamazoo Brothers	
	None	Tests	Tests, Ed, Higher Ed, BA	None	Test Scores	Higher Ed, Test Scores	Age	None
White	8.321	[3.474]	6.076					
Father's Education	1.517	1.042	[.399]	1.00	.63	[.19]		[.4641]
Father's Occupation	.211	.167	[.069]	.136	.120	.060		[.0016]
Father White Collar								5.7330 [2.5506]
Father Native Born								[.7176] [2.1803]
Father Absent	[-1.777]	-2.256	[-.518]					[-4.9389]
# of Siblings								-1.1987
Mother's Education				.64	.40	[.09]		.7022 [.3497]
Family Income				.87	.67	[.22]		
Non-Farm	6.581	4.706	4.413					
Non-South	-.379	-3.630	[-2.590]					
Father's Education^2								[.0100]
Father's Occupation^2								
Siblings^2								.1994
S.D. of Residual	21.910	20.422		21.99	20.88	17.99		21.840
R^2	.128	.244		.112	.200	.406		.125

	Kalamazoo Brothers	
	Age, Test Scores	Age, Test Scores, Ed, Higher Ed,BA
White		
Father's Education	[.1852]	[-.3852]
Father's Occupation	[-.0420]	[-.0367]
Father White Collar	5.713	[1.148]
Father Native Born	[.8289]	[2.3511]
Father Absent	[-1.847]	[1.469]
# of Siblings	[-.5723]	[-.0235]
Mother's Education		
Family Income		
Non-Farm		
Non-South		
Father's Education^2	[.0288]	[-.0431]
Father's Occupation^2		
Siblings^2	[.1144]	[.0985]
S.D. of Residual	20.311	18. 381
R^2	.244	.384

Notes to Table 2.3

[Coefficients in brackets are less than twice their standard error.]

a. The NORC brothers and Talent brothers surveys are excluded from this table because they have such small sample sizes that most measured characteristics have insignificant effects on men's Duncan scores, even without test scores or schooling controlled.

b. The SRC coded occupation in PA and PSID surveys only in broad occupation categories.

c. Sewell and Hauser (1975), pp. 80-81.

d. Family income at age 16, inflated to 1967 purchasing power, coded in dollars.

than men with native-born fathers. This result also holds for the OCG, though the regression is not shown in Table 2.3. In OCG, the advantage disappears once one controls current region and city size.

We tested for non-linearities by adding a quadratic term for each family background measure. In order to make linear coefficients comparable across samples, all non-linear terms were constructed to be uncorrelated with the analogous linear term.[1] No background characteristic had consistently non-linear effects across all our national surveys, but several non-linearities were significant ($p < .05$) in more than one sample. For instance, the negative impact of additional siblings on a man's Duncan score diminished as the number of siblings increased. This did not show up in the PA analyses, probably because SRC grouped all men with eight or more siblings. In two of the three surveys that measure father's Duncan score, the positive impact of an increase in father's Duncan score declines as the father's Duncan score increases. This non-linear effect was in the same direction in the NLS bivariate analysis, but was not significant. And in all but one survey (PSID) the impact of an extra year of father's education on expected Duncan score was larger at higher levels of schooling.

The five large national surveys of mature men did not measure the same background characteristics, so it is difficult to explore interactions between background characteristics in systematic fashion. We tested for interactions in two ways. First, we ran separate regressions for whites and non-whites and for men with white collar, blue-collar, and farm fathers. Chapter 8 summarizes the white-nonwhite differences. The Appendices show the equations for white collar, blue collar, and farm men. In addition, we tested the significance of specific multiplicative interactions between race, father's education, father's occupation, and number of siblings. No multiplicative interaction between two background variables was significant ($p < .05$) in more than one survey, and adding multiplicative terms never increased the explanatory power of background characteristics (R^2) by more than 0.003. This suggests that little information is lost by pooling groups from diverse backgrounds, such as whites and non-whites.

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[1] For a more detailed discussion of this procedure, see Chapter 12.

Corrections to the Demographic Model

A model which defines family background as a set of measured socioeconomic characteristics may underestimate the influence of family background effects either by omitting important variables or by inadequate measurement of included variables.

The analyses reported in Table 2.3 seldom include all potentially relevant measures of background. Bowles (1972) argues, for example, that parental income is an important determinant of men's statuses. Parental income is available only in the Wisconsin and OCG-II surveys. These two surveys also provide data on mother's education. When we added parental income to the OCG-II equation in Table 2.2, R^2 rose from 0.208 to 0.219. Adding mother's education raised R^2 to 0.226. The pattern in the Wisconsin sample is similar, though the values of R^2 are lower because background is less varied and respondents are younger.

The analyses reported here also omit birth order. In OCG, older sons had higher Duncan scores than younger sons. But this correlation diminished sharply when total family size was controlled. With family size controlled, the effects of birth order were U-shaped. Eldest siblings scored about 0.9 Duncan points above next-to-eldest siblings. Men with two or more older siblings also scored above men with only one older sibling from families of the same size. Having an older rather than a younger sibling raised a man's expected Duncan score by about 0.4 points. Adding these birth order variables to the regression of occupational status on background measures increased R^2 by only about 0.002. In what follows we therefore ignore birth order.

Greeley (1975) argues that ethnic identity and religious affiliation affect men's chances of economic success. Greeley has synthesized data from several surveys which collected information about ethnic and religious identity.

Greeley assigned respondents to one of fourteen major ethnic/religious groups: Blacks, Jews, French Catholics, German Catholics, German Protestants, Irish Catholics, Irish Protestants, Italian Catholics, Polish Catholics, Slavic Catholics, Spanish surname Catholics, British Protestants, and "American" Protestants. Race accounts for 5 percent of the variance in Duncan scores in Greeley's sample; adding the other thirteen ethnic categories raises R^2 by 3 percent. Some of these ethnic and religious effects are probably traceable to other background traits such as father's occupation, family size, or region of birth. It would be surprising if adding these ethnic and religious variables to the equations shown in Table 2.3 raised R^2 by more than 0.02.

These results suggest that if we took account of all the background measures discussed above, along with their non-linearities and interactions, we might be able to explain as much as 28 percent of the variance in occupational status in 1962 and 25 percent in 1973.

More accurate measurement of background characteristics might further increase the explanatory power of background. OCG II provides considerable data on measurement error. CPS resurveyed about 1000 OCG-II respondents by telephone a month after the initial data collection. Both the initial survey and the telephone reinterview asked about father's education, father's occupation, and family income when the son was 16. Bielby, Hauser, and Featherman (1976) used this sample to test several competing models of response error. Bielby et al conclude from their analyses that measurement error in reports of social background is random for non-blacks. When they regressed non-blacks's occupational status on their parental income, father's education, and father's occupation, R^2 was 0.176. When they corrected for random measurement error, R^2 rose to 0.225. Most of this increase was due to eliminating errors in the reporting and coding of the respondent's current occupation, not his parental characteristics.

Matters may, however, be more complex than Bielby et al's data imply.

Chapter 13 suggests that Bielby et al's procedure for estimating the reliability of father's education and occupation yields higher reliabilities than alternative procedures. If we use the reliabilities implied by correlating two brothers' reports on their father, or the reliabilities implied by the correlation between reports of father's education and father's occupation, R^2 might be appreciably higher than 0.225. On the other hand, Bielby et al's correction for random error in the respondent's report of his own occupational status is too high for our purposes, since Bielby et al did not allow for real changes in occupation between their two interviews. Nor did they exclude non-respondents. Instead, they assigned non-respondents a Duncan score. Furthermore, Bielby et al's predicted values depend entirely on father's education, father's occupation and parental income, which are likely to contain a lot of error, whereas ours depend partly on race and number of siblings, which probably contain less error.

These considerations make it difficult to say how much variance in occupational status we could hope to explain if we had reliable measures of demographic background. Chapter 13 suggests that our measure of the respondent's occupational status had a reliability of 0.86 and that our measures of demographic background had reliabilities ranging from 0.80 to 0.95. Because there is substantial overlap among these background measures, the reliability of their weighted sum (i.e., an individual's predicted occupational status, or $\hat{Y}$) should exceed the average reliability of the separate background measures. While no precise estimate is possible without knowing the reliability of race, region of upbringing, number of siblings, and the like, we selected the value 0.91 as a plausible guess. This implied that if we had had reliable measures of demographic background and 1973 occupation, we might have gotten an R^2 on the order of $(0.25)/(0.86)(0.91) = 0.32$.

In theory, this calculation implies that family background explains at least 32 percent of the variance in 25-64 year old men's current occupational statuses. In practice, matters may not be quite this simple.

We began by defining family background as everything that differentiated men with one set of parents from men with another set of parents. Demographic background characteristics obviously differentiate families from one another, but they can also create variation within families. A family that has a high income when one son reached college age, for example, may have a low income when another son reaches the same age. Similarly, a family that is intact when one son is growing up may be broken by the time another son grows up, and a family that lives in the South when one son is growing up may move North by the time another son reaches the same age. In principle, these temporal variations in the demographic characteristics of a given family could have a significant effect on children's life chances. Yet so far as we know, no one has actually demonstrated the existence of such effects. We will therefore assume that such effects are unimportant. If this assumption is correct, the variance explained by demographic background constitutes a minimum estimate of the overall explanatory power of family background.

Mechanisms by which Demographic Background Affects Occupational Status

Family background may affect occupational status for several reasons:

(1) Men from advantaged backgrounds may have cognitive or non-cognitive skills or they may have educational credentials that employers value.

(2) Among men with similar skills and credentials, those with advantaged parents may apply for jobs in higher status occupations than those with disadvantaged parents.

(3) Among men with similar skills and credentials who apply for a given job, employers may favor those with advantaged parents.

Men from advantaged backgrounds have higher average test scores than men from less advantaged backgrounds, but Table 2.3 shows that parental advantages have a substantial impact on occupational status among men with equal test scores (see the PSID, Talent, Veterans, Kalamazoo, and Wisconsin results.) The samples differ in so many respects that one cannot draw any firm general conslusions about the fraction of the overall impact of background explained by test scores, but it seems clear that family background does not exert its impact on occupational status primarily by affecting the cognitive skills measured by standard tests.

Parents can also influence a son's economic chances by inculcating useful personality traits, such as leadership, creativity, and aggressiveness in their sons. The Talent survey measured some of these traits in adolescence. Chapter 5 shows that adolescent test scores account for about 40 percent of the effect of demographic background on occupational status in the Talent sample, while test scores and non-cognitive traits together account for close to 60 percent.

Parental advantages increase a son's occupational status mainly by increasing his educational attainment. Thus the benefits uniquely associated with having a highly educated father, having a father with a white collar rather than a blue collar job, having a foreign born father, being raised in an intact family, having a small number of siblings, and growing up in the North rather than the South all become negligible once we control the respondent's own education. The benefits associated with having a father with a high Duncan score show a less consistent pattern. They become negligible in the NLS, Talent, and Veterans samples, but only fall by about half in the Wisconsin, OCG, and OCG-II samples. (For OCG-II results, see Bielby et al., 1976.) This inconsistency in the degree to which a father's occupation affects a respondent's occupation by affecting his education is not due to age differences between samples or to the other control variables. The Wisconsin sample is the same age as the Talent sample and both control test scores. The OCG sample also has essentially the same control variables as the NLS, and while OCG covers a wider age range, the coefficient of father's occupation is even larger in OCG samples restricted to men over 45 than in the 25-64 year old sample. Fortunately for our argument, however, the Talent, Veterans, and NLS coefficients have quite large sampling errors, and could be as large as the OCG and OCG-II values. The latter are unlikely to be as low as the Talent and Veterans values since the OCG samples are quite large. The OCG coefficients might fall appreciably if we could control test scores, but this does not happen in the Talent and Veterans samples. Our provisional conclusion is therefore that father's occupational status has a moderate direct effect on a son's occupational status, independent of his education and cognitive skills. 2/

2/ Blau and Duncan (1967), Jencks et al. (1972), and Duncan, Featherman and Duncan (1972) all relied on OCG data and all concluded that father's occupation had an appreciable effect even with respondent's education controlled. Sewell and Hauser (1975) found smaller absolute effects.

Farm upbringing and black skin reduce men's occupational status in all our surveys, even with the linear and non-linear effects of schooling controlled. But farm rearing has a negligible effect once a man's current region and type of community are controlled. This suggests that farm boys suffer occupational disadvantages because they tend to remain in farm communities. Race is the only background characteristic that consistently influences Duncan scores with everything else controlled.

These results suggest that among men with any given amount of education, background plays a very modest role in determining occupational status. This suggests that (a) background has little impact on occupational choice once education is controlled, and (b) employers choose among job applicants primarily on the basis of the applicants' characteristics, not their parents' characteristics. The principal exception is race. Either blacks apply for jobs in lower status occupations than whites, or else employers favor white applicants over blacks with similar amounts of schooling. This remains true when we control test scores. Employers may, of course, favor whites because they have non-cognitive attributes that make them more valuable employees than whites with similar test scores, education and demographic backgrounds. We suspect, however, that race per se is the trait that concerns employers.

The Effects of Unmeasured Background Characteristics.

We defined family background as everything that made the children of one set of parents different from the children of another set of parents. We then estimated the impact of "demographic" background on occupational status. This set a lower bound on the overall impact of family background. But families with precisely the same demographic characteristics can obviously differ in important respects. They may, for example, raise their children differently. Or they may have different occupational aspirations for their children. Different families also pass on different genes to their children. If employers favor applicants with certain genes, genotype will affect eventual occupational status. Since brothers share roughly half their genes, this will make brothers' occupations more alike than random individuals' occupations.

Unfortunately, we cannot disentangle the effects of genetic resemblance between brothers from the effects of **their common environment.** If we wanted a clean estimate of the impact of parental **and community characteristics, independent** of genetic factors, we would need data on the degree of resemblance between adopted children reared in the same home. If we wanted a clean estimate of the overall impact of genetic factors, we would need data on identical twins reared in random homes. No such data are currently available, so the impact of genotype on occupational status remains problematic.[3] This ambiguity is not peculiar to research on brothers. If one uses occupational resemblance between fathers and sons to estimate the effect of family background, for example, one must also consider the possibility that such resemblance is genetic in origin. If certain genes play a consistent role in occupational advancement over several generations, the fact that sons get half their genes from their fathers will produce a correlation between fathers' and sons' statuses. One way around this problem would be to investigate the effects of fathers' occupations on the occupational status of their adopted children, but no such data are currently available.

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[3] Taubman (1976) has tried to sort out genetic and environmental influences on occupational status using comparisons of identical and fraternal twins reared together, but this requires a number of problematic assumptions (see Taubman, forthcoming).

Our definition of "family background" is therefore conceptually ambiguous. It includes whatever genes brothers have in common, plus whatever non-genetic influences they have in common. We cannot say for sure that any specific genetic or non-genetic influence fits this description perfectly. Nonetheless, resemblance between brothers has considerable intuitive appeal as a measure of the overall impact of family background.

The NORC, Talent and Kalamazoo brothers' surveys provide data on brothers. Rows 5, 6, and 7 of Table 2.1 describe these samples. The appendices provide more detail. Measured parental advantages explain less of the variance in men's Duncan scores in the Talent (R^2=0.141) and Kalamazoo (R^2=0.125) surveys than in the large national surveys. The difference is probably due to the character of the Kalamazoo and Talent samples. Kalamazoo respondents all attended school in Kalamazoo. The variance of race, farm origins, region and parental education is therefore restricted. Talent respondents all reached 11th grade, which again implies some restriction on the variance of background characteristics. The low explanatory power of measured background in the Talent and Kalamazoo surveys suggests that the intraclass correlation between brothers' Duncan scores may also be lower in these two samples than in a representative national sample.

suggests that the intraclass correlation between brothers' Duncan scores may also be lower in these two samples than in a representative national sample.

In our one national sample of 25-64 year old brothers, namely the NORC brothers sample, a regression equation similar to that in Table 2.2 explains 18.9 percent of the variance in occupational status, compared to 20.8 percent in OCG-II. The difference between the NORC brothers and OCG-II could easily be due to random sampling error, but

[illegible],

also respondents with brothers may be somewhat less affected by family background than respondents without brothers. In order to test this latter hypothesis, we separated OCG respondents with brothers from those without brothers. The respondents without brothers naturally come from smaller families, and none is an only child. Demographic background characteristics explain about a tenth more variance in education, occupational status, and earnings for men without brothersthan for men with brothers. If this also holds for OCG-II, R^2 for OCG-II respondents with brothers should be almost identical to R^2 for NORC brothers. Nonetheless, the correlation between brothers may underestimate the impact of family background on occupational status in the population as a whole.

Another possible source of bias in these samples is that large numbers of men with brothers cannot (or occasionally will not) tell an interviewer how to locate their brother. The NORC survey relied entirely on data supplied by one brother to locate the other. The Kalamazoo survey used other informants as well, but a pair's chances of inclusion were still higher if they knew each other's addresses and telephone numbers. Other things equal, we would expect brothers who resembled one another in terms of education, occupational status, and income to keep in closer contact than brothers who had ended up very different in these respects. To test this expectation, the NORC and Kalamazoo surveys asked each respondent for his brother's education, occupation and earnings, even if he could not provide an address or telephone number. As expected, the intraclass correlations between respondents' reports on themselves and their brothers was slightly higher in the NORC survey if the respondent provided sufficient information for NORC to locate his brother than if the respondent was unable to provide such information. The differences never approached statistical significance, however. This suggests if we rely only on brothers who have both been contacted directly, we may slightly overestimate the effect of family background on life chances. In what follows we will assume that this bias offsets the bias due to omitting men without brothers.

The intraclass correlation between brothers' Duncan scores is 0.371 ± 0.076 for the NORC sample. The intraclass correlations for the two more restricted samples are lower: 0.321 ± 0.097 for the Talent sample, and 0.309 ± 0.051 for the Kalamazoo sample. We will assume a population value of 0.37 for US men 25-64. If we correct for random measurement error (see Chapter 13), the implied true correlation is about 0.37/0.86=0.43.

One can get similar data for larger and more representative samples if one is willing to accept a respondent's report of his brother's occupation. Olneck (1976) found that respondents do not give very accurate information regarding their brothers' occupation. But he also found that intraclass correlations obtained in this way did not differ systematically from correlations between independent reports. The same is true in the NORC brothers sample. If this pattern holds more generally, we can legitimately augment our three surveys with other surveys that merely asked respondents about their brothers.

As part of a 1964 national survey of attitudes toward child-rearing (Kohn, 1969), NORC asked respondents about their brothers'occupations. These data are still not available to independent investigators, but Melvin Kohn, the principal investigator, made several tabulations for us. Unfortunately for our purposes, he scaled occupations using the Hollingshead scale. He then averaged the respondent's brothers' Hollingshead scores. One can only estimate the intraclass correlation from such data if one knows the number of brothers on whom each respondent reported. For the 529 men with one brother, the estimated intraclass correlation of Hollingshead scores was 0.409. For the 510 men with two brothers', the intraclass correlation was at least 0.374. The intraclass correlations for the 819 men with three or more brothers appear to be less than 0.37. But since we do not know whether these men reported on all their brothers , we cannot calculate the exact intraclass correlations.[4]

A second source of data is Hodge's report (in correspondence) that the correlation between a respondent's report of his own occupation and his report of his oldest brother's occupation was 0.298 in a NORC national survey of unspecified size. We do not know whether Hodge derived this statistic from Kohn's survey, which was conducted by NORC, or from another source. One likely reason for the difference between Hodge's findings and Kohn's is that Hodge scaled occupations using Hodge, Siegel and Rossi's prestige rankings, not Duncan scores or Hollingshead scores. The Hodge-Siegel-Rossi prestige scale (also known as the NORC scale) generally yields lower correlations than the Duncan scale between occupational position and other traits (see Duncan, Featherman, and Duncan, 1972). After correcting for this, the implied correlation between brothers' observed Duncan scores is about $0.298/0.86 = 0.347$[5].

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[4] Kohn recorded the total number of brothers, not the number for whom the respondent provided usable occupational data. If respondents failed to report on one or more brothers, this estimated r_I will be too low. The bias grows potentially more serious as the number of brothers rises.

[5] Jencks et al (1972) relied on Hodge's data, but failed to make this correction, an error pointed out by Hauser and Dickinson (1974).

Since the amount of error in men's reports on their brothers is unknown, and since not all errors are necessarily random, these sibling correlations are not completely comparable to sibling correlations obtained from two independent informants. But is we assume somewhat more random error in men's report on their brothers than in their reports on themselves, the Hodge and Kohn results seem consistent with the intraclass correlation of 0.37 obtained in the NORC brothers survey. We will treat this as a maximum estimate of the effect of shared background on men's occupational statuses in the early 1970's.

If the demographic background measures discussed earlier could have produced R^2's around 0.25 in the early 1970's, and if brothers did not influence one another, an intraclass correlation of 0.37 implies that background characteristics not measured in our surveys must have explained at least 12 percent of the variance in 25-64 year olds' occupational statuses. The observed difference between the percentage of the occupational variance explained by measured background and the intraclass correlation between brothers' occupations is 0.182 in the NORC sample, 0.180 in the Talent sample, and 0.184 in the Kalamazoo sample. Had these regressions included measures of ethnicity, religion, mother's education and parental income, the expected discrepancy would fall to around 0.14. We assume that this discrepancy would fall to 0.12 if we could eliminate sampling bias in favor of brothers with similar occupations. Using our crude correction for measurement error, the discrepancy is 0.43-0.32=0.11. But if brothers influence one another, these calculations overstate the importance of unmeasured background.

Mechanisms by Which Unmeasured Background Affects Occupation Status

We will explore the nature of these unmeasured background factors by constructing four hypothetical models which account for sibling resemblance in occupational attainment. To estimate these models we assume that family effects are symmetric and that brothers do not directly affect one another. Later we discuss alternative assumptions. Table 2.4 lists the sibling correlations used to estimate these models.

When we look at measured background characteristics, those that provide an advantage for one outcome (e.g. education) tend to provide a similar advantage for other outcomes (e.g. occupation). Using the background characteristics measured in OCG, for example, predicted values of education and occupation correlate 0.970. This means that measured parental advantages can be adequately described using a single "vertical" dimension, equivalent to what sociologists call "SES." But this is less true when we consider unmeasured background. Suppose we construct three hypothetical variables: one (F_Q) to account for the resemblance between brothers' test scores, a second (F_U) to account for the resembalnce between brothers' schooling, and a third (F_Y) to account for sibling resemblance on occupational status. Each of these hypothetical variables is a weighted sum of all the measured and unmeasured background characteristics that brothers share. The weights are the regression coefficients these characteristics would have when predicting a given outcome. F_U is thus identical to $\hat{U}$ where $\hat{U}$ includes unmeasured as well as measured traits. Figure 2.1 displays the expected correlations among these measures, assuming that a respondent's traits influence one another, but do not influence his brother. The correlation between F_U and F_Y (i.e., a in Figure 2.1) are about 0.91 in both samples of men over 30. This is slightly lower than the OCG correlation between

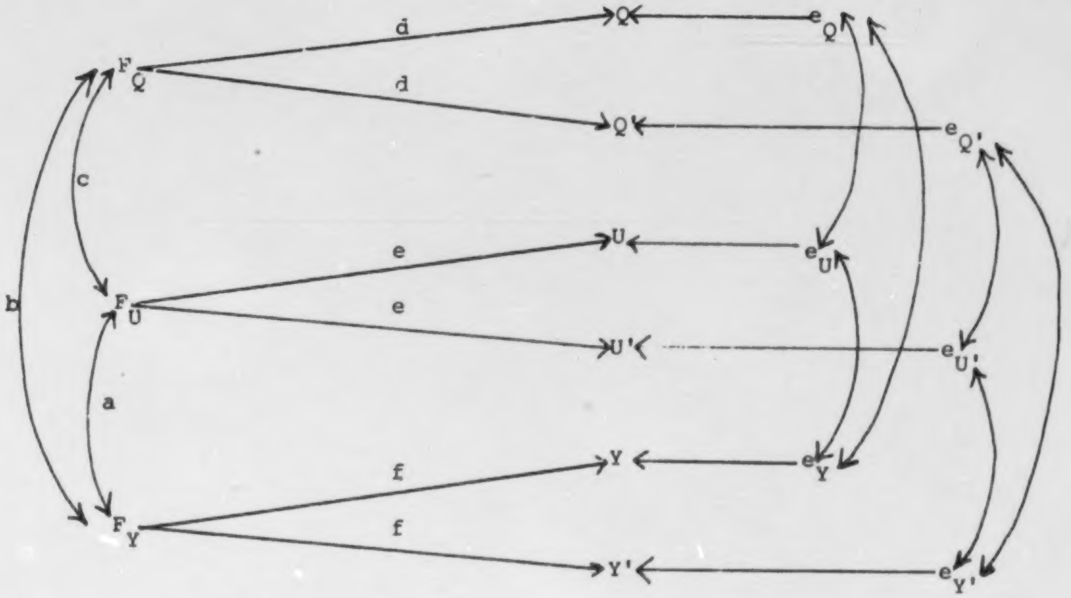
Table 2.4: Correlations between brothers' characteristics: NORC (top row), Kalamazoo (middle row), Talent (bottom row).

	X	Q	U	Y	X'	Q'	U'	Y'
X	1.000							
	1.000							
	1.000							
Q	...	1.000						
	.260	1.000						
	.387	1.000						
U	.339	....	1.000					
	.383	.576	1.000					
	.371	.632	1.000					
Y	.338	....	.595	1.000				
	.218	.453	.591	1.000				
	.356	.484	.706	1.000				
X'	...	....	....	....	...			
	.775[a]	.253	.389	.218	1.000			
	.797[a]	.419	.409	.373	1.000			
Q'	....	...	...	...	...	...		
	.253	.469	.400	.300	.260	1.000		
	.419	.580	.451	.360	.387	1.000		
U'	.347	...	.528	.401	.339	...	1.000	
	.389	.400	.549	.378	.383	.576	1.000	
	.409	.451	.546	.417	.371	.632	1.000	
Y'	.321	...	.401	.371	.338	...	.595	1.000
	.231	.300	.378	.309	.218	.453	.591	1.000
	.373	.360	.417	.329	.356	.484	.706	1.000

note: X = respondent's report of father's occupational status at 15 (Duncan score)
Q = test score prior to school completion
U = highest grade completed
Y = occupation (Duncan score)
Primes denote the second member of a given pair. All correlations were computed from files in which every pair appears twice, with order reversed. This makes product-moment correlations equal to intraclass correlations.

[a] In a few cases where reports of father's occupation were missing, the report of a brother was substituted. Therefore, the correlation between X and X' is slightly higher than the reliability coefficient. In the NORC sample, only the first respondent was asked father's occupation.

Figure 2.1



Where: Q = test score
U = schooling
Y = Duncan score
Primes denotes second brother, with all pairs arranged randomly.

Estimated values:

	a	b	c	$d(=\sqrt{r_{QQ'}})$	$e(=\sqrt{r_{UU'}})$	$f(=\sqrt{r_{YY'}})$
NORC	.906	NA	NA	NA	.727	.609
Kalamazoo	.918	.788	.788	.685	.741	.556
Talent	.983	.822	.801	.762	.739	.574

This model is just identified. The relevant equations standardized are:

1) $r_{QQ'} = d^2$

2) $r_{UU'} = e^2$

3) $r_{YY'} = f^2$

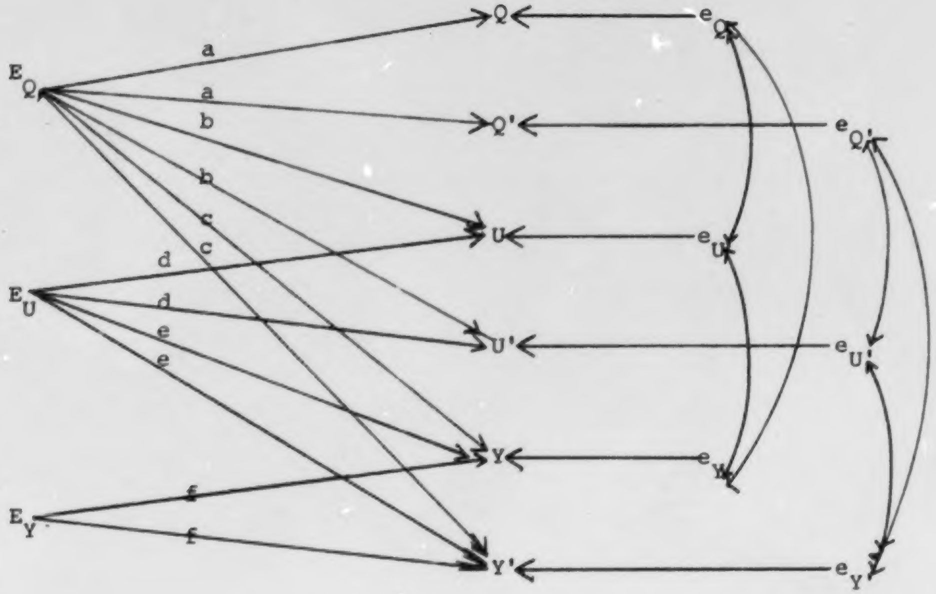
4) $r_{QU'} = cde$

5) $r_{QY'} = bdf$

6) $r_{UY'} = aef$

These equations can be solved using the sibling correlations in Table 2.4.

Figure 2.2



Q = test score
U = schooling
Y = Duncan score

	INCLUDING E_Q						OMITTING E_Q		
	a	b	c	d	e	f	d	e	f
NORC	NA	NA	NA	NA	NA	NA	.727	.552	.257
Kalamazoo	.685	.584	.438	.456	.268	.213	.741	.510	.221
Talent	.762	.592	.472	.442	.311	.097	.739	.564	.104

This model is just identified. The relevant standardized equations are:

INCLUDING E_Q

1. $r_{QQ'} = a^2$
2. $r_{UU'} = b^2 + d^2$
3. $r_{YY'} = c^2 + e^2 + f^2$
4. $r_{QU'} = ab$
5. $r_{QY'} = ac$
6. $r_{UY'} = bc + de$

OMITTING E_Q

1. $r_{UU'} = d^2$
2. $r_{YY'} = e^2 + f^2$
3. $r_{UY'} = de$

These equations can be solved using the sibling correlations in Table 2.4

$\hat{U}$ and $\hat{Y}$, which was 0.97 when $\hat{U}$ and $\hat{Y}$ embodied only demographic measures. Evidently unmeasured background is not quite as one-dimensional as measured background.

We can test the significance of this "second" dimension of background by estimating the model in Figure 2.2. Like Figure 2.1, Figure 2.2 contains three hypothetical background variables. But these hypothetical background variables are now defined as uncorrelated. The first, E_Q is defined as all background factors that influence test scores. E_U represents all family traits that affect schooling and that are uncorrelated with family traits that influence test scores. E_Y is defined as all family traits that influence occupational status and that are uncorrelated with family traits that influence schooling and/or test scores. E_Q, E_U, and E_Y can now all influence Y. "f" is the standardized coefficient of E_Y in an equation predicting occupational status, with E_Q and E_U controlled. Since E_Q , E_U and E_Y are orthogonal, f^2 also measures the increment to R^2 from adding E_Y to the equation. Background characteristics that are uncorrelated with the characteristics that influence schooling account for 6.6 percent of the variance in Duncan scores for NORC men, 4.9 percent for Kalamazoo men, and 1.1 percent for Talent men. Background traits that are uncorrelated with traits that influence either test scores or schooling account for 4.5 and 0.9 percent of the variance in Duncan scores for Kalamazoo and Talent men respectively. These increments, although small, are significant for the two samples of men over 30.[6] At least some of the unmeasured characteristics that make brothers alike on occupational status differ from those that make brothers alike on schooling. We cannot say whether these differences are due to the differential effects of genes, home environment, community, or other factors.

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6. "f^2" measures the increment to R^2 from adding the second dimensions of background. Since we know the N's for each sample, we can calculate the significance of the additional dimension.

Next, we investigate how much effect unmeasured family background traits have on a son's Duncan score once we control test scores and schooling. If the unmeasured background traits that account for sibling resemblance on occupational status are similar in character to the measured traits investigated earlier, they should affect men's status largely by affecting the amount of schooling men acquire. Figure 2.3 displays a model for testing this hypothesis. The model has two hypothetical variables: one (H_U) which accounts for brothers' resemblance on schooling and another (H_Y) **which accounts** for brothers' resemblance on Duncan scores over and above what would be expected on the basis of similar educational attainments. H_Y can be defined as the weighted sum of all family traits that influence occupational status with education controlled; "c" is the standardized coefficient of H_Y when predicting occupational status with education controlled.[7]

Background influences on occupational status are much smaller among men with equal amounts of schooling than among men in general. But at least for men over 30, background still has modest effects on occupational status that are not mediated by schooling or test performance. Among men with equal amounts of schooling, a one standard deviation difference in background is associated with a difference in Duncan scores of (0.361) (24.194) = 8.7 points for NOPC brothers, **the difference is 6.3 points** for Kalamazoo brothers and 3.5 points for Talent brothers.[8]

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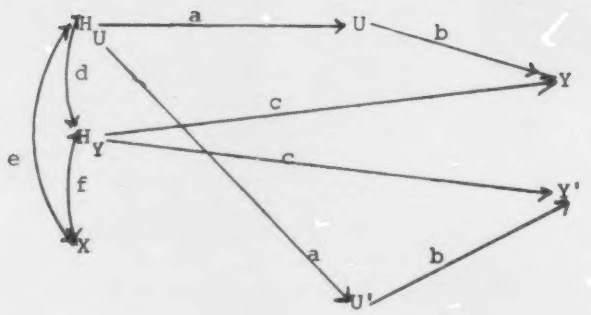
[7] The significance of c is the same as the significance of the increment in R^2 when we add H_Y to the regression of occupation on education.

8/ H_Y is only weakly correlated with father's occupation for Kalamazoo men i.e., f=0.133. This is probably a sample artifact. The correlation between fathers' and son's Duncan scores is only 0.224 in the Kalamazoo survey. This is considerably smaller than the correlation observed in the other surveys of men over 30 examined in this chapter. The other correlations ranged from 0.290 in the PSID to 0.402 in the OCG.

Note that the correlations involving father's occupation are not corrected for measurement error and do not utilize the brother's report of the father's occupation, which is available in Kalamazoo and Talent samples. The correlation between brothers' reports is analogous to a reliability coefficient (see Chapter 13). We can use this correlation to estimate the correlation between reported and true values for father's occupation. Ignoring the handful of pairs where only one brother reported his father's occupation and one value was used for both brothers, the implied correlation between measured and true values is 0.87 for Kalamazoo and 0.88 for Talent. We can estimate the correlations of hypothetical variables with the father's true occupational status by dividing the values shown in Figure 2.3 for e and f by 0.87 or 0.88. The implied value of f for Talent is thus almost unity.

If we take the correlations involving X' as well as X in Table 2.4 into account, the model is over-identified. In principle, this calls for a maximum-likelihood solution. In practice, the fit obtained from an algebraic solution is quite good, so maximum-likelihood solutions are unnecessary.

Figure 2.3



Where: H_U = all family traits that make brothers alike on schooling.

H_Y = all family traits that influence Duncan scores, net of schooling.

U = years of school

Y = respondent'sDuncan score

X = Duncan score of respondent's father

	a	b	c	d	e	f
NORC	.727	.411	.361	.701	.466	.549
Kalamazoo	.741	.472	.273	.587	.516	.133
Talent	.739	.637	.141	.657	.502	.861

This model is just identified. The equations are:

1. $r_{UU'} = a^2$

2. $r_{YY'} = b\, r_{UU'} + acd$

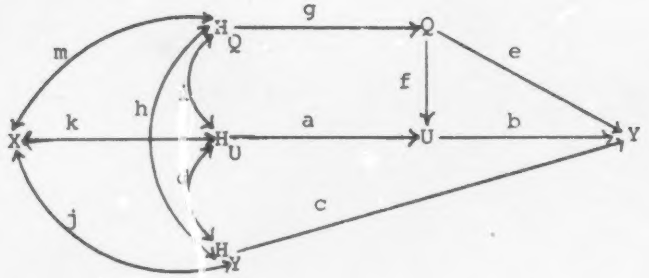
3. $r_{YY'} = b\, r_{YU'} + c^2 + abcd$

4. $r_{YU} = b + abcd$

5. $r_{XU} = ae$

6. $r_{XY} = cf$

Figure 2.4



Where: Q = respondent's test score
U = respondent's schooling
Y = respondent's Duncan Score
X = Duncan score of respondent's father
H_Q = all family traits that influence test scores
H_U = all family traits that influence schooling, net of test scores
H_Y = all family traits that influence Duncan scores, net of schooling and test scores

	a	b	c	d	e	f	g	h	i	j	k	m
Kalamazoo Brothers	.579	.413	.246	.489	.151	.331	.685	.379	.617	.078	.513	.379
Talent Brothers	.515	.625	.130	.522	.029	.431	.762	.607	.512	.868	.396	.508

Note: The model has been simplified visually by omitting the second brother.

Controlling both schooling and test scores does not reduce the effects of background much more than controlling schooling alone did. The model pictured in Figure 2.4 allows us to isolate those background factors that influence Duncan scores independently of test scores and schooling. This model includes three hypothetical variables. H_Q accounts for brothers' resemb lance on test scores. H_U accounts for resemblance between brothers on education attainment, over and above what would be expected from test score similarity. H_Y accounts for resemblance between brothers' Duncan scores over and above what would be expected from similarity on test scores and schooling. H_Y has almost as much effect on occupational status after controlling both H_U and H_Q as it had in the previous model which is controlled only H_U.

Conclusions about Unmeasured Background Characteristics

About 18 percent of the variance in occupational status among men over 30 is due to background factors not measured in the Kalamazoo and NORC brothers surveys. These unmeasured traits do not affect occupational status in the same way as measured background characteristics. The demographic advantages that maximize education are almost the same as those that maximize occupational status, suggesting that demographic advantages are essentially one dimensional. This is slightly less true when we look at unmeasured background characteristics. If we look at brothers over thirty, the unmeasured background characteristics that affect occupation are not quite the same as those that affect education and test scores. In addition, the unmeasured background characteristics that affect education have more effect on occupational status with test scores and schooling controlled than do demographic traits. As a result a one standard deviation difference in overall background leads to a 6 to 9 point difference in expected Duncan scores among men over 30 even when they have equal test scores and schooling.

We can think of several ways in which unmeasured background could influence occupational status net of schooling and test scores.

(1) Brothers share half their genes. Genes may affect men's careers independently of test scores and education, e.g. by affecting physical stamina, physical and mental health, physical attractiveness, or even personality traits. If genes affect men's careers in such ways, brothers may end up more alike in terms of occupational status than we would expect on the basis of their test scores and education.

(2) Parental child-rearing practices may differ in ways that affect either men's occupational preferences or their chances of succeeding in the occupation they prefer. Some parents may, for example, encourage obedience and dependability while others encourage independence and creativity. This may affect the sons' careers independent of their educational attainment. Kohn (1969) shows that such child-rearing practices are largely independent of demographic advantages such as those we measured. This might be less true if we had better data on fathers' jobs (Wright, 1976), data on their capital assets (Bowles, 1972), or data on their ethnicity and religion (Greeley, 1976), but even families that were alike in all these respects would surely raise their children quite differently in many cases.

(3) Parental values may be partially independent of demographic background. Within any given demographic group, some families may raise their children to believe that status is the most important goal in life while others may stress money or an easy life. Families that value status should also tend to value schooling, since many believe that schooling leads to a prestigious job. If this were the case, we would expect the unmeasured

parental characteristics that maximize occupational attainment independently of schooling to be very similar to those that maximize schooling itself This is largely but not completely true.

(4) Brothers grow up in the same communities and usually attend the same schools. Schools and communities may make brothers alike in values, aspirations, or other non-cognitive traits (such as sense of "fate control") independent of their parents. The demographic characteristics included in our surveys (e.g., non-farm, non-south) would presumably capture some of these community differences, but not all of them. Jencks and Brown (1975) found, however, that controlling parents' demographic characteristics explained most of the school-to-school variance in both occupational status and career plans at 23. Their data suggest that school and community characteristics account for less than five percent of the variance in young men's later occupational statuses. Since unmeasured family characteristics seem to account for at least 12 percent of the variance in mature men's status, family-to-family differences within a given community must account for at least 7 percent, and perhaps more.

(5) Men raised in a given community often remain there. If both brothers remain, and if their community has a distinctive occupational structure, this in itself will make the brothers more alike in occupational status than random individuals from similar demographic backgrounds but different cities. Again, however, Jencks and Brown's data suggest that growing up in one community rather than another cannot account for most of the resemblance between brothers that is independent of demographic background.

All the path models discussed above assume that family effects on brothers' test scores, schooling, and occupations are symmetric and that the characteristics of one brother do not affect the characteristics of another brother. Other assumptions are equally plausible. For instance, families may pay more attention to first-born sons. Or parents may try to equalize children's opportunities by devoting more time and money to their less gifted children. Or a younger brother might emulate his older brother's educational aspirations. Or the actual schooling of an older brother may affect the younger brother's plans.

The Kalamazoo sample is large enough to investigate whether birth order influences how family effects operate. Olneck (1976) investigated this by estimating four equations predicting test scores, education, occupation and earnings separately for older and younger brothers. Only one of the 24 regression coefficients in these equations differed significantly for older and younger brothers. No direct effects of position on any outcome were significant. Olneck concluded that his evidence did not support the hypothesis that demographic advantages affect older and younger siblings differently. We made a parallel investigation using OCG data and again failed to find significant interactions between demographic background and birth order in equations predicting occupational attainment.

We do not have enough information to test the assumption that the characteristics of one brother directly affect those of another brother. To do this we would need to measure all the background characteristics that influence occupational status. Then we could estimate a man's Duncan score as a function of his background, his individual characteristics, and his brother's characteristics. Since we do not know all the family traits that affect occupational attainment, we cannot separate parental effects on occupational status from reciprocal influences.[9]

General Conclusions

Our analyses lead to the following conclusions about the effects of family background on men's occupations:

1. Conventional demographic background measures explained no more than a third of the variance in eventual occupational attainment among men aged 25 to 64 in 1973, even after correcting for measurement error. Father's education, father's occupation and family size appear to have slightly non-linear effects on occupational prestige, but the evidence for this is not very consistent. The relationship between measured background and occupational status is adequately captured using additive assumptions. The demographic advantages that raise occupational status are almost identical (r=0.97) to those that raise educational attainment.

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[9] We could also estimate reciprocal influences if we knew that background had fewer dimensions than outcomes. If we assume that only one family background factor affects both education and occupation, for example, a model in which one brother's education affects the other's education will be just identified, at least so long as brothers' occupations do not affect one another. This model fits the conventional wisdom better than the ones presented above, but it yields implausible results (e.g., coefficients greater than 1.00) in the one sample for which we tried it (Kalamazoo).

2. Demographic advantages raise occupational status largely because men from advantaged backgrounds acquire more schooling than men from less advantaged backgrounds, and schooling influences occupational prestige. But non-whites work at lower status jobs than whites with the same amount of schooling.

3. Demographic measures do not capture the full impact of coming from one family rather than another. Unmeasured advantages that are independent of demographic background probably account for about 12 percent of the variance in occupational status.

4. These unmeasured advantages exert considerable impact on a man's eventual occupational status even with schooling controlled. The families in which brothers both do better than one would expect on the basis of their schooling are usually but not always those in which both brothers have already gotten more schooling than average. Since the correlation is not perfect, there must be some background characteristics that raise eventual occupational status but do not affect years of schooling.

5. Our data do not tell us whether the unmeasured background characteristics that affect occupational status are genetic, environmental, or both. Neither do they tell us whether they are characteristics of parents, communities or both. Jencks and Brown's data suggest, however, that parental characteristics are more important than community characteristics.

Chapter 3: Effects of Family Background on Individual Earnings

By Mary Corcoran

Men with socially and economically privileged parents usually earn more as adults than men with less privileged parents. There is no reason to suppose that men with privileged parents have a stronger preference for cash as against psychic income from their work. If anything, the contrary seems likely. It follows that men with privileged parents either have or acquire characteristics that make them more productive than average, benefit from positive discrimination by employers, or search more effectively for highly paid jobs. This chapter tries to assess the importance of each of these factors.

I explore background effects on earnings using 10 surveys described in Table 3.1. I will begin by defining background as a set of 13 demographic characteristics. Then I will review the likely effects of other demographic variables and of measurement error. Finally, I will expand my definition of background to include all background factors shared by brothers.

Effects of Measured Background on Men's Earnings

Earnings differentials between men from different backgrounds depend to a considerable extent on how you define background. I will begin with the same set of family characteristics as in chapter 2: race, father's birthplace, father's education, mother's education, father's occupation, parental income, the region in which the children are raised, whether the children grow up on a farm, the number of other children, and whether the parents stayed together until the child was at least 16.

These analyses pool results for all racial and socioeconomic groups. We tested for multiplicative interactions of race, father's occupation, and family size with all other independent variables. If these interactions were more than twice their standard error in a given survey, we retained them in the wage equation for that survey. In addition, we estimated separate [redacted] for whites and non-whites in all surveys with enough non-whites to yield stable results. Chapter 8 discusses white-nonwhite differences. The Appendices also present separate equations for men with white-collar, blue-collar, and farm fathers. There are no consistent differences.

Table 3.1 shows the regressions of LnEarnings on our thirteen demographic background measures. No two regressions use precisely the same background measures so the results are not strictly comparable. In addition, the dependent variable in OCG and OCG-II is Ln Income, not Ln Earnings. This income measure is grouped into standard CPS categories. The earnings data are grouped in the Kalamazoo and Veterans samples. The use of income rather than earnings inflates the variance of the dependent variable. Grouping reduces the variance of the dependent variable. The standard deviation of grouped income is thus about the same as the standard deviation of ungrouped earnings. Using income rather than earnings hardly affects correlations. Contrary to what one would expect if relationships were bivariate normal, grouping increases the correlation of income or earnings with virtually all independent variables. Other things equal, then, R^2 should be slightly higher in the OCG, Veterans and

Table 3.1: Relationships Between Ln Earnings and Family Background for Civilian Non-Institutional Non-Student Males with Positive Earnings and Complete Data.

ırvey ganization ear of arnings ata)[a] 1)	Survey Name (2)	Additional Restrictions on Sample (3)	Age in Earning Year (4)	Sample Size (5)	Background Measures[b] (6)	Test Score (7)	S.D. of Ln Earnings (8)	S.D. of Predicted Ln Earnings[c] (R^2's are in parentheses) (9)	S.D. Predicted Family Means[d] (Sibling r) (10)
PS 1961)	Occupational Changes in a Generation (OCG)	Includes students & men with earnings = 0 (if income > 0)	24-64[h]	11,504	1, 3,4,5, 7,8,9,10	No	.819[e]	.334[e] (.166)	
RC 1964)	Productive Americans (PA)	Heads of households	24-64[h]	1,188	1,2,3, 7,8,9	No	.707	.311 (.194)	
RC 1971)	Panel Study of Income Dynamics (PSID)	Heads of households	25-64	1,774	1,2,3,4,5, 7,8,9,10	Yes	.753	.258 (.117)	
PS 1972)	Occupational Changes in a Generation (OCG-II)	Same as OCG	24-64[h]	15,817	1,3,4,5,6, 7,8,9,10,11	No	.684[e]	.225[e] (.108)	
ORC 1973)	NORC Brothers	Brothers	24-64[h]	300	1,3,4,5, 8,9,10	No	.870[j]	.220 (.064)[g]	.312 (.129)
PS 1964)	Veterans Survey	Veterans	30-34	803	1,3,4, 7,8,10	Yes	.498[j]	.175 (.124)	
SD 1966)	Parnes' National Longitudinal (NLS)	None	44-59[i]	2,580	1,2,3,4, 5,7,8,10	No	.883	.343 (.151)	
alent 1972)	Talent 28 Year-Olds	Men who reached 11th grade	28(±1)	839	1,3,4, 9,10	Yes	.396[f]	.071[f] (.032)	
alent 1971-2)	Talent Twins & Siblings	Brothers who reached 11th or 12th grade	28(±1)	198	1,3,4, 9,10	Yes	.406[f]	.069[f] (.029)	.185[f] (.207)
lneck 1973)	Kalamazoo Brothers	Brothers who reached 6th grade in Kalamazoo, Michigan	35-59	692	(1),2,3,4, 5,6,(7), (8),9,10	Yes	.446	.126 (.080)[g]	.209 (.220)

Notes to Table 3.1 (continued):

[h] Respondents were aged 25-64 at the time of the survey. They were asked their earnings for the previous calendar year. This means that some respondents were only 24 during the earning year.

[i] Respondents were 45-59 at the time of the survey, but see note h.

[j] Earnings grouped using standard CPS categories. The top category is "$15,000 or Over" for veterans in 1964. It is "$25,000 or Over" for NORC Brothers in 1973-74.

Kalamazoo samples than in the Census, PA, PSID, NLS, and Talent samples.[1]

The first five lines of Table 3.1 cover our five national surveys of men aged 25-64. They show a fairly steady decline over time in the percentage of variance in earnings attributable to background measures. The apparent decline in R^2 is not conclusive, however, since the five surveys do not use the same background measures. To get better evidence on time trends, I looked at identical regressions from OCG and OCG-II. Table 3.2 displays the results. Every regression coefficient is smaller in absolute size for 1972 than 1961; and the non-farm coefficient becomes negative in 1972. This is partly because the standard deviation of Ln Earnings is smaller in 1972 than 1961, but the zero-order correlations are also uniformly smaller. It seems safe to conclude that the effect of family background on income fell between 1961 and 1972. The change is particularly conspicuous in the case of race. In 1961, whites earned $e^{.5746} = 1.78$ times more than non-whites from similar backgrounds. By 1972 whites earned only $e^{.2741} = 1.32$ times more than non-whites from similar backgrounds.[2]

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[1] McClelland discusses many of these issues in more detail in Chapter 16.

[2] Those unfamiliar with semi-log income functions may find some explanation helpful. Since the regression coefficient of a variable represents its effect on the natural log of earnings, a unit increase in the independent variable multiplies earnings by the anti-log of the coefficient. The text illustrates this logic. Furthermore, as k approaches zero, e^k approaches $1 + k$. This means that small regression coefficients can be interpretted as percentage effects. In OCG, for example, the coefficient of Father's Education is 0.0172. This means that with other background measures controlled a one year increase in Father's Education is associated with $e^{.0172} = 1.0173$ times more income. Thus each extra year of father's education increases income by 1.7 percent. The degree of equivalence between coefficients and percentage effects can be seen in the following calculations: $e^{.01} = 1.010$; $e^{.05} = 1.051$; $e^{.10} = 1.105$; $e^{.20} = 1.221$; $e^{.30} = 1.350$; $e^{.40} = 1.492$.

Table 3.2

Regressions of Ln Income on Family Background Characteristics for Males 24-64 and with Non-Zero Incomes in 1961 (OCG) and 1972 (OCG-II)

		White	Father's Education	Father's Duncan Score	Number of Siblings	Non-Farm Upbringing	Father White Collar	Father Absent at 15	Non-South Upbringing	R^2 (S.D. of Residuals)
	B	.5746	.0172	.0041	-.0176	.2271	[.0445]	-.1101	.1676	.165
CG	(S.E.)	(.0252)	(.0021)	(.0006)	(.0024)	(.0183)	(.0272)	(.0196)	(.0164)	(.749)
	r	.280	.219	.253	-.184	.191	.211	-.077	.216	
	B	.2904	.0122	.0027	-.0166	.1384	[.0150]	-.0826	.0922	.095
G-II	(S.E.)	(.0191)	(.0015)	(.0004)	(.0021)	(.0135)	(.0193)	(.0202)	(.0118)	(.650)
	r	.187	.195	.217	-.168	.187	.173	.045	.153	

Note: Both equations use grouped income data as the dependent variable.

I also looked at the OCG-II results using ungrouped income. The use of ungrouped data lowers all correlations by about a tenth, raises the standard deviation of the dependent variable by about a tenth, and therefore leaves the unstandardized regression coefficients essentially unchanged.

Table 3.3 shows the results when Ln Earnings is regressed on all the measured background variables available in each of our samples of mature men. Parental advantages have consistently positive effects on men's earnings. The only surprise is that men with foreign-born fathers earn 10 to 16 percent more than men with American-born fathers from otherwise similar backgrounds. This is consistent with the findings in chapter 2 regarding occupational status. This advantage disappears in OCG once we control region of residence and community size, but even then second generation immigrants are at no disadvantage.[3]

Since no two surveys measured precisely the same background characteristics in the same way, it is difficult to compare multivariate coefficients across surveys. But two differences are striking. The negative effect of Southern birth declined sharply in the early 1960's. As we note later, Southern birth influences earnings largely because men reared in the South are likely to remain there, and Southern workers earn less than their Northern counterparts. The observed decline in effects of Southern rearing is at least partly caused by the decline in earnings differences between North and South. If more men had moved out of their region of birth in 1972 than in 1961, this would also reduce the coefficient of region of birth. We have not analyzed this issue in detail, however. The negative effect of race declined quite steadily throughout the period[4]

Past research has often assumed that family background characteristics affect Ln Earnings in linear and additive ways. We explored non-linearities and interactions. We looked for non-linearities by entering a quadratic term for each variable. We tested for interactions by adding multiplicative interaction terms between variables and by running regressions separately for sons of white-collar, blue-collar, and farm fathers.

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3. Father Foreign was inadvertently omitted from most of our OCG tabulations.
The assertions in the text are based on subsequent tabulations.

4. See Schwartz and Williams, Chapter 9, for a time series on racial differences in income.

Table 3.3: Regression Coefficients of Background Variables with Various Controls.[a]

1) urvey	(2) Controls	(3) White	(4) Father Born in U.S.	(5) Father's Education	(6) Father White Collar	(7) Mother's Education	(8) Raised Outside the South	(9) Not Raised On a Farm	(10) Number of Siblings	(11) Father's Occupation (Duncan Score)	(12) Living with Father At 15	(13) R^2	(14) S.D. of Residuals
CG 1961)	None	.5688		.0171	[.0431]		.1690	.2261	-.0179	.0041	-.1073	.166	.749
	Education	.4862		[.0018]	[-.0012]		.1208	.1797	[-.0041]	.0028	[-.0360]	.229	.720
A 1964)	None	.4309	-.1131	.0390			.0905	.2788	-.0185			.194	.637
	Education	.3294	-.1394	[.0116]			[.0240]	.1856	[.0006]			.303	.594
LS 1966)	None	.3992	-.0974	.0367	[.0903]		[.0334]	.3736		[-.0018]	-.1397	.151	.815
	Education	.2680	-.1070	.0122	[.1038]		[-.0144]	.2591		[-.0010]	[-.0053]	.237	.774
SID 1971	None	.3578	-.1634	.0242	.2100		.0906	.1752	-.0266	[-.0022]	[-.1095]	.117	.709
	Test Scores	.2337	-.1349	.0167	.1831		[.0544]	.1440	-.0167	[-.0027]	[.0908]	.172	.687
	Test Scores & Educ.	.1989	-.0989	[.0019]	[.0685]		[.0152]	.1404	[-.0023]	[-.0026]	[.0374]	.255	.653
CG-II 1972)	None	.2741		[.0021]	[.0080]	.0123	.0866	.1301	-.0129	.0017	[.0200]	.108	.646
ala-azoo 1975)	Age		[.0135]	[.0100]	.1226	[-.0060]			-.0180	[.0007]	[-.0383]	.080	.431
	Age, Test Scores		[-.0129]	[.0054]	.1222	[-.0088]			[-.0077]	[.0001]	[.0114]	.164	.412
	Age, Test, Education		[-.0068]	[-.0003]	[.0784]	[-.0112]			[-.0020]	[-.0001]	[.0554]	.206	.401

[a] These equations also include significant quadratic and interaction terms, whose coefficients are not shown. These terms were constructed to be uncorrelated with the related background variables so that the linear, additive regression coefficients could be compared across samples.

Almost all background traits had essentially linear effects on Ln Earnings. The one exception was family size. Additional siblings after the first always lower a man's expected earnings, but the negative impact decreases as the number of siblings increases.[5] While a few other non-linearities were significant in at least one survey, none had a consistent sign across all the samples, and none was highly significant in more than one sample.

Since different surveys measured different background characteristics, the multivariate results are not strictly comparable. But no multiplicative interaction between different parental characteristics was consistently significant across surveys or even had a consistent sign.[6] Nor did splitting the sample show consistent differences in returns to schooling between men with white-collar, blue-collar, or farm fathers. Returns to a year of education were higher for sons of white-collar men than for sons of blue-collar men in PSID, NLS, Talent, and Kalamazoo surveys, and the difference was significant in three of these surveys. But in the OCG, sons of white-collar men received significantly lower returns to each year of schooling than did sons of blue-collar men. In all these split sample runs, we used three education variables, (years of schooling, years of schooling past high school and college graduation) to distinguish the non-linear effects of education from true interactions between background and education.

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[5] The Siblings^2 term was significant and positive only in the OCG and Kalamazoo analyses. Siblings^2 was insignificant in other surveys -- but these surveys grouped high values of Siblings.

[6] There was one limited exception. The coefficient of the interaction between father's education and father being foreign born was positive and significant in two of the three surveys that contained measures of father's birthplace and father's education.

Column 9 of Table 3.1 shows the standard deviation of predicted earnings for each sample. This indicates the degree of inequality we might expect if the parental advantages and disadvantages measured in these surveys were the only source of inequality in sons' earnings. The standard deviation of predicted incomes was 0.334 in 1961. Men whose parents ranked in the least advantaged five percent of the population had incomes 41.8 percent of the national average in 1961. Men whose parents ranked in the least advantaged 20 percent had incomes 60.4 percent of the national average. Those whose parents ranked among the most privileged 20 percent typically earned 48.0 percent more than the national average, while those whose parents ranked in the most privileged five percent earned 74.6 percent more than the national average. We do not have analogous figures from OCG-II, but the overall standard deviation of predicted values fell from 0.334 to 0.225, a decline of about a third. If the shape of the distribution remained roughly constant, predicted values must have been only two-thirds as far from the mean in 1972 as in 1961. The typical value for the top five percent would therefore be (0.225/0.334)(74.6) = 50.3 percent more than the mean instead of 74.6 percent more.

These analyses do not include all conceivably relevant measures of background advantages. Bowles, for instance, argues that parental income is an important determinant of sons' economic status. OCG-II indicates that the correlation between men's current incomes and their retrospective reports of their parents' income when they were 16 is only 0.22, and that the standardized regression coefficient after controlling the other traits shown in Table 2 is only 0.11.

This estimate would be biased downward if there is a great deal of measurement error in retrospective reports of parental income. Bielby, Hauser, and Featherman (1976) obtained a correlation of 0.913 between the mailback questionnaire and telephone reports of parental income for 578 non-black males aged 20-64 in the OCG-II reinterview project. After analyzing the overall pattern of correlations they concluded that the reliability of the mailback questionnaire item was 0.86. Of course, respondents might consistently misreport their parents' income. But Sewell et al's survey of Wisconsin high school graduates includes a measure of parental income derived from four years of state tax records. A 100 percent difference in average parental income when the son was of college age was associated with an 8 percent difference in the son's earnings at age 31.[7]

Birth order is widely presumed to influence men's economic status. But while birth order has a substantial effect on men's earnings in OCG, this effect disappears when family size is controlled.

Greeley's (1976) ethnic-religious data, described in chapter 2, indicates that race accounts for 5 percent of the variance in family income, and that adding thirteen other ethnic-religious categories raises R^2 by 3 percent. The PSID also asked respondents about religious affiliations. Mueser did not include this measure in his analyses, but Greg Duncan has investigated the influence of religion on earnings using a somewhat different subsample than the one in this volume. Duncan found that both Catholic and Jewish men enjoy an earnings advantage over Protestant men, even when these men come from families with otherwise similar demographic characteristics. Adding dummy variables for Catholic and Jewish affiliation to the regression of Ln Hourly Wages on father's occupation, mother's occupation, family size, region of birth, farm origins and work experience raises R^2 from 0.149 to 0.166 for white male heads of household aged 25 to 55. Taubman (1976) also found that being Jewish significantly raised 1969 wages and that being Protestant significantly lowered 1969 wages for 47 year old Air Force veterans. Since Duncan was able to raise R^2's by 0.017 using only two broad measures of religious affiliation, a fuller range of religious and ethnic measures would probably raise R^2's by 0.02 or 0.03.

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[7] Bivariate regression estimated from data in Hauser and Daymont (1976).

More accurate measurement of father's education, mother's education, and especially the father's job characteristics might also increase the explanatory power of parental status. As noted in chapter 2, there is a great deal of controversy about the magnitude of errors in retrospective reports of parental traits. Bielby et al. (1976) found that allowing for random reporting error increased R^2 from 0.176 to 0.225 when occupational status was regressed on father's education, father's occupation, and parental income for white men. Since income data seem to be about as reliable as occupation data, eliminating measurement error should increase R^2 by about the same percentage in regressions of income on these same background measures. This may be too large an adjustment, however, since many of the background measures in these analyses (e.g. family size and race) are probably measured with less error than father's education, occupation and income.

Measured background accounted for about 17 percent of the variance in Ln Earnings in the early 1960's and about 11 percent of the variance in Ln Earnings in the early 1970's. These R^2's might increase to 20 and 14 percent if we added religious affiliation and ethnic background to our background measures. Correcting for random measurement error might raise the R^2's to 26 and 19 percent. Thus all the parental advantages we have considered so far accounted for about one-quarter of the variance in Ln Earnings in the early 1960's and about one-fifth of the variance in Ln Earnings in the early 1970's.

Mechanisms by which Background Affects Earnings

The measures of background examined in this volume affect men's earnings largely by affecting their test scores, occupational aspirations, and educational attainment. Assessing how much of the effect of family background is exerted via test scores is difficult,since only one of the five large nationally representative surveys (PSID) contains a test score measure and this measure is quite crude.[9] In this survey, however, 30 to 40 percent of the benefits associated with being white, having a highly educated father, coming from a small family, and growing up outside the South were due to the fact that these advantages were associated with high test scores.[10] This pattern also held for the smaller and less representative samples of Veterans, Talent 28 year olds, and Kalamazoo men aged 35 to 59, though not all these samples had data on all these background measures.[11]

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[9] Mueser discussed how this measure was obtained in Appendix D. The measure depends in great part on one's familiarity with cliches. Also, this measure was obtained at the same time as were earnings data, so it may have been influenced by variations in amount of education and by occupation as well as by family background.

[10] The benefits of a particular background variable were calculated holding all other background variables constant. This misestimates the overall effects of those background variables that are causally prior to others. For instance, race affects earnings partly by affecting father's education, father's occupation and family size. The overall benefits of being white include these effects. My method does not include them.

[11] Many of these background variables were initially insignificant in the smaller studies, but the direction and magnitude of effects were similar to those observed in the PSID survey. There was one exception. Controlling test scores only reduced returns to father's education by 20 percent for Talent 28-year olds.

Test scores allegedly measure cognitive skills. Non-cognitive skills may also be important determinants of economic success. For instance, parents might increase son's earning power by encouraging their sons to develop traits like "autonomy" or "aggressiveness." The Talent and Kalamazoo surveys provide measures of such psycho-social factors prior to entering the labor market. Talent respondents were asked about parental and peer encouragement and about their own occupational aspirations in the eleventh grade. Talent respondents were also asked to rate themselves on a number of personality dimensions and were asked about their high school activities. The Kalamazoo survey obtained teacher ratings of students on nine personality traits in tenth grade. These non-cognitive measures have appreciable effects on earnings but they do not depend to any great extent on background. As a result, controlling the Talent non-cognitive measures only reduced the effects of background traits on Ln Earnings at 28 by about a seventh.[12] In Kalamazoo, the reduction is even less (Olneck, 1976). These results suggest that parents do not increase sons' chances of economic success primarily via the psycho-social variables analyzed in this volume. This conclusion must be qualified, however, given our uncertainty about whether we examined the right non-cognitive variables.

Family background affects men's earnings primarily by affecting the amount of education men acquire. Table 3.3 shows how controlling education altered coefficients of the family background measures in each survey.[13] Controlling educational attainment makes the coefficients of father's education, father's occupation, number of siblings, and coming from a broken home extremely small and usually insignificant. Southern birth and farm upbringing exert some effect even among men with similar test scores and education, but these effects became negligible in OCG once we controlled the size of the community and region in which the respondent lived in 1962. . This suggests that employers pay very little attention to whether a man is Southern or farm born. Such men are handicapped partly because they get less education and have lower test scores, and partly because they usually remain in communities where everyone is paid less than in the urban North. The same holds for having a foreign born father, which has a significant positive coefficient in all our large samples, but whose effect disappeared in OCG once we controlled current community size. Having a father with a high status occupation increases earnings significantly among men with equal amounts of schooling in the OCG survey. This remaining effect might disappear if we could control test scores; unfortunately OCG did not collect test scores.[14]

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[12] Mueser created a composite background measure for his analysis. Its standardized coefficient was 0.225 with no controls vs 0.189 with all the Talent non-cognitive traits controlled.

The only background characteristic that has a substantial effect on earnings with everything controlled is race. Race is, of course, not just a characteristic of the respondent's parents but a visible characteristic of the respondent himself. The fact that employers paid blacks less than whites with the same amount of education (and the same test scores in the PSID) strongly implies that in this instance employers engaged in "pure" discrimination.

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[13] Three surveys are not included in Table 3.2:NORC Brothers, Veterans, and Talent. The NORC Brothers are excluded because no measure of family background had a significant coefficient when Ln Earnings were regressed on the family background variables. The Veterans and Talent surveys were excluded because the respondents were young and the range of education restricted.

[14] The regression coefficient of father's occupation net of schooling was not significant in any of the other national surveys (NORC, PSID, and NLS). Small sample sizes might explain this for NORC. PSID only coded father's occupation into broad categories.

The background measures analyzed in this volume are primarily parental status measures. We include only one ethnic/religious measure, and that is race. Perhaps other dimensions of ethnic and religious identity influence earnings independently of schooling and test scores. Some evidence supports this hypothesis. Greg Duncan found that Jewish men earned 18.5 percent more than other men even after controlling other aspects of family background, schooling, work experience and test scores in his PSID sample of white male heads of household aged 25 to 55. Taubman found that for his sample of white 47 year old Air Force veterans, being Jewish significantly increased 1969 wages and being Protestant significantly reduced 1969 wages, net of test scores, schooling and measured background.[15]

Controlling occupation did not further reduce returns to demographic measures of family background once education was controlled. Controlling occupation reduced returns to being white by less than one-fifth and reduced returns to non-farm rearing by less than one-sixth for men with the same family background, test scores (where available), education and experience.

It appears that employers hire and pay men largely on the basis of those men's own characteristics--not on the basis of their parents' characteristics. Once we controlled education and test scores, family background traits other than race had very small effects on men's earnings. Family advantages do not increase men's earnings by providing access to the more prestigious occupations, since controlling occupational status had no effect on wage returns to background traits once education was controlled--again with the exception of race. Despite the widespread public concern expressed over racial inequalities in economic opportunity, there still remains a substantial racial difference in earning power between whites and non-whites. This cannot be explained by racial differences in education, test scores, or occupational status.

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[15] Taubman (1976).

Effects of Unmeasured Family Background on Earnings

Until now I have defined background as a set of measured parental advantages, such as race or father's education. If these were the only parental characteristics that affected a child's earnings, we would expect children who had had the same measured advantages to end up as much alike as children who were actually raised in the same home. To determine whether this is true, we have looked at three surveys of brothers (Talent Siblings, Kalamazoo, and NORC Amalgam). Table 3.1 describes these surveys. Even allowing for a secular decline in the impact of parental status, measured parental advantages explain less of the variance in earnings in these samples of brothers than in the larger national samples from the same years (PSID, OCG-II). In the case of the NORC brothers, this is probably just a matter of random sampling error. In the case of the Kalamazoo brothers, the target population may be atypical.

The Talent brothers are only 28, and no variable correlates as well with earnings at 28 as with later earnings. Whatever its causes, the low explanatory power of measured background characteristics implies that the correlation between brothers' earnings in these three samples may be lower than it would be in a representative national sample.

The correlation between the logarithms of brothers' earnings was 0.13 for NORC brothers. The 95 percent confidence interval for this correlation runs from -0.03 to 0.29. Our other surveys suggest that the correlation between the logarithms of brothers' earnings is at the upper end of this interval. The correlation was 0.22 for Kalamazoo brothers. The correlation for the log of hourly earnings was 0.21 for Talent brothers. John Brittain obtained an intraclass correlation of 0.44 between the logarithms of family income for 151 men from 66 families in the Cleveland area. Paul Taubman obtained a correlation of 0.30 between the logarithms of DZ twins' earnings in a national sample of nearly 1000 DZ twins. Of course, DZ twins probably end up more alike than ordinary brothers. These figures therefore suggest that the intraclass correlation between the logarithms of brothers' earnings is unlikely to exceed 0.25. Corrections for random reporting errors might increase this to 0.28. Further, the **PSID data suggest that at least 29 percent of** the variance in annual earnings of **25 to 64 year** olds is due to random year-to-year fluctuations and **systematic effects of experience. The intraclass correlation between brothers'** total earnings from 25 to 64 could thus be as high as 0.28/(1-.29)=0.39.

Unless brothers affect one another, the intraclass correlation between their earnings is equal to the percentage of variance we would be able to explain if we regressed earnings on all the genetic and environmental factors that brothers had in common. The intraclass correlations are at least twice as large as the observed R^2 in each of our samples of brothers. This suggests that unmeasured background characteristics exert as much impact on earnings as the measured characteristics discussed earlier. In order to analyze the reasons for resemblance between brothers we need a full correlation matrix such as the one in Table 3.4. If the unmeasured family characteristics that produced resemblance between brothers were similar in character to the measured characteristics

produced

Table 3.4

Correlations between brothers' characteristics: NORC (top row), Kalamazoo (middle row), Talent (bottom row).

	X	Q	U	Z	X'	Q'	U'	Z'
X	1.000 1.000 1.000							
Q	... .260 .387	1.000 1.000 1.000						
U	.339 .383 .371	... .576 .632	1.000 1.000 1.000					
Z	.124 .200 .096	... .366 .356	.355 .409 .366	1.000 1.000 1.000				
X'	[a] .775[b] .797[b]	[a] .253 .419	[a] .389 .409	[a] .197 .094	1.000 1.000 1.000			
Q'	.... .253 .419	... .469 .580	... .400 .451	... .169 .216	... .260 .387	1.000 1.000 1.000		
U'	.341 .389 .409	... .400 .451	.528 .549 .546	.171 .269 .210	.339 .383 .371	... .576 .632	1.000 1.000 1.000	
Z'	.174 .197 .094	.... .169 .216	.171 .269 .210	.129 .220 .207	.124 .200 .096	... .366 .356	.355 .409 .356	1.000 1.000 1.000

Note: X = respondent's report of father's occupational status at 15 (Duncan score)

Q = test score prior to school completion

U = Highest grade completed

Z = Ln Earnings

Primes denote the second number of a given pair. All correlations were computed from files in which every pair appears twice, with order reversed. This makes product-moment correlations equal to intraclass correlations.

[a] The NORC survey did not ask the second brother about his father's occupation, so we used the first brother's report for both respondents.

[b] In a few cases where reports of father's occupation were missing, we substituted the report of a brother. Therefore, the correlation between X and X' is slightly higher than the reliability coefficient.

ert most of their influence indirectly, via test scores and educational attainment. But this is not the case. If the NORC brothers' earnings were alike solely because their educational attainments were alike, for example, the correlation between their earnings would be only 0.067, not 0.129.[16] If test scores and education together accounted for resemblance between brothers' earnings, the correlation between Kalamazoo brothers would be 0.113, not 0.220, and the correlation between Talent brothers would be 0.099 not 0.207.[17] This means that unmeasured background characteristics must affect earnings in some way that is independent of test scores and educational attainment.

When we look only at measured background characteristics, those that provide an advantage for one outcome (e.g. education) tend to provide a similar advantage for other outcomes (e.g. occupation and earnings). Using the background characteristics measured in OCG, the predicted values of Education, Occupation and Ln Earnings correlate 0.89 to 0.98, for example.[18] This suggests that parental advantages can be fairly adequately described using a single "vertical" dimension equivalent to what sociologists have called SES. This is less true when one considers unmeasured aspects of family background. Suppose we again construct three hypothetical variables. One (F_Q) accounts for brothers' resemblance on test scores, a second (F_U) accounts for brothers' resemblance on educational attainment, and a third (F_Z) accounts for brothers' resemblance on earnings. Figure 3.1 presents the presumed correlations between these hypothetical variables visually. In our three studies of brothers the three hypothetical variables correlate 0.5 to 0.8. Clearly one dimension of background cannot fully account for brothers' resemblance on test scores, education, and earnings. Note that the model pictured in Figure 1 assumes that brothers do not directly affect one another.

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[16] If there is no partial correlation between brothers' earnings with their education controlled, $\hat{r}_{ZZ} = r_{ZU}r_{UU'}r_{U'Z'} = r^2_{ZU}r_{UU'}$.

[17] If we use p's to represent partial regression coefficients, $\hat{r}_{ZZ'} = p_{ZU}r_{UZ'} + p_{ZQ}r_{QZ'}$. The values of p_{ZU} and P_{ZQ} can be obtained by regressing Z on Q and U simultaneously.

[18] If we use $\hat{U}$ to denote predicted education, $\hat{Y}$ to denote predicted occupation, and $\hat{Z}$ to denote predicted Ln Income, the values are $r_{\hat{U}\hat{Y}} = 0.970$, $r_{\hat{U},\hat{Z}} = 0.886$, and $r_{\hat{Y}\hat{Z}} = 0.925$.

We next ask to what extent the family traits that account for brothers' similarity on test scores and educational attainment also explain the similarity between brothers' earnings? We can determine this by estimating the model in Figure 3.2. In this model, E_Z represents all family traits that affect a son's earnings and that are uncorrelated with those family traits that affect a son's test scores and schooling. "f" is the standardized coefficient of E_Z in an equation predicting Ln Earnings and controlling E_X and E_U . f^2 represents the percentage of the total variance in Ln Earnings attributable to background characteristics that do not affect education of test scores.[19]

We could, of course, reverse this model and ask to what extent the family traits that account for earnings similarity between brothers also account for educational similarity? This model is pictured in Figure

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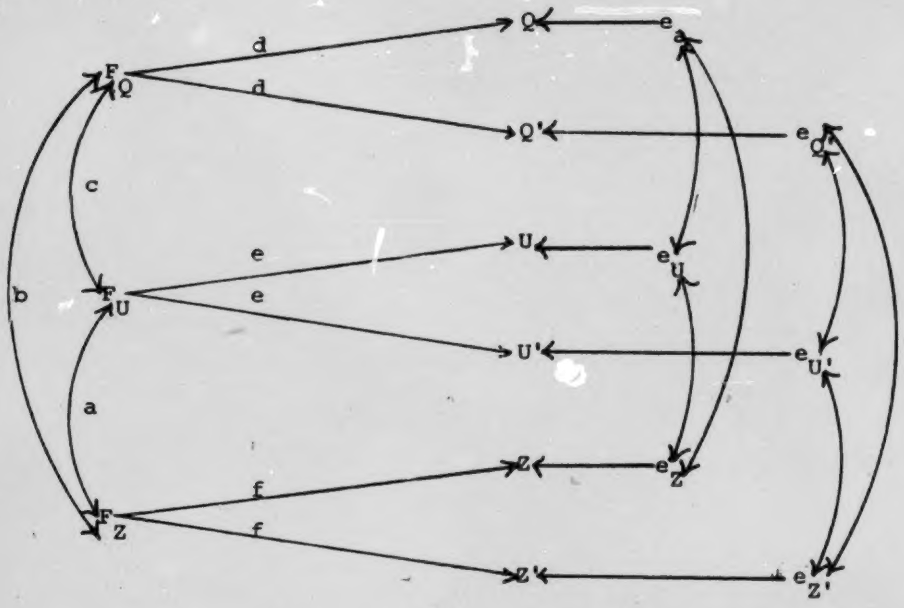
[19] Since the NORC Brothers and Taubman's twins have no test score measure, "f" really represents the effect of those family characteristics that are uncorrelated with the family traits that influence education.

Figure 3.1



Where :

Q = Test Scores;

U = Education;

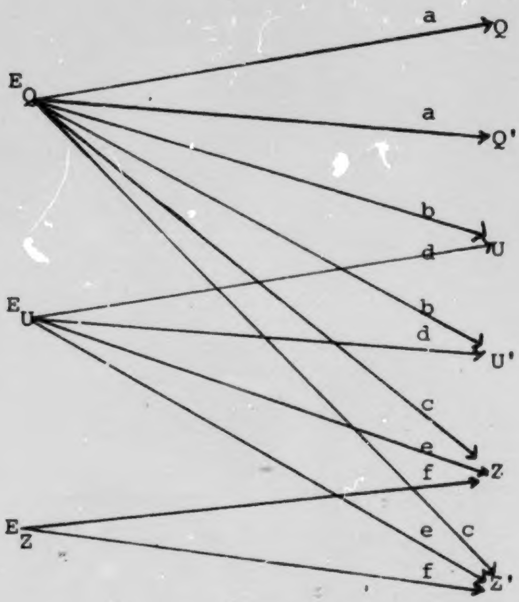
Z = Ln Earnings; Primes denote the second person in a pair.

F_Q, F_U, F_Z, = Weighted sums of background traits that fully account for sons' resemblance on test scores , education and Ln Earnings, respectively.

Equations are identical to those in Figure 2.1 , with Z and Z^1 substituted for Y and Y^1.

Solutions for:	a	b	c	d	e	f
NORC Brothers	.655	NA	NA	NA	.728	.359
Kalamazoo Brothers	.773	.520	.789	.685	.741	.455
Talent Brothers	.607	.643	.802	.762	.739	.469
Taubman DZ Twins[al]	.72	NA	NA	NA	.73	.55

Figure 3.2



Where: Q,U,Z,Q',U',Z',E_Q have the same meanings as in Figure 3.1.

E_U = hypothetical family traits that are uncorrelated with E_Q but that influence schooling and Ln Earnings.

E_Z = hypothetical family traits that are uncorrelated with E_Q and E_U and that influence Ln Earnings.

The relevant equations appear in Figure 2.2

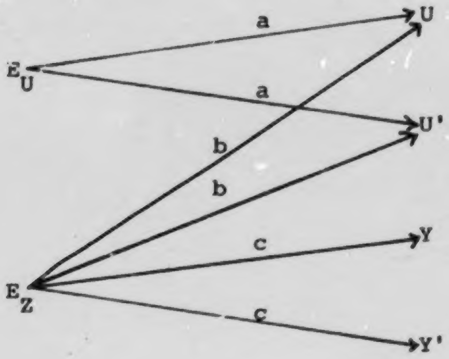
Solutions for:	Omitting E_Q		Including E_Q	
	f	f^2	f	f^2
NORC Brothers	.261	.069	NA	NA
Kalamazoo Brothers	.297	.089	.294	.086
Talent Brothers	.355	.126	.392	.117
Taubman DZ Twins	.380	.144	NA	NA

3.3. The high values of "a" in this model indicate that in addition to the family characteristics that influence men's earnings, other family characteristics have an independent influence on schooling. It seems reasonable to conclude that the family characteristics that maximize sons' test scores and educational attainment are not the same as those that maximize sons' earnings.

As noted above, families' unmeasured characteristics affect their sons' earnings in some way that is independent of the son's test scores and educational attainment. This was not true of measured parental advantages. Suppose we construct a model which allows us to isolate the family factors that produce these "direct" effects on earnings. This model also includes three hypothetical variables--one (H_Q) to account for resemblance between brothers' test scores, another (H_U) to account for resemblance between their educational attainments (over and above what would be expected on the basis of test scores similarity), and a third (H_Z) to account for resemblance between earnings (over and above what is expected on the basis of similar test scores and education). This model is pictured in Figure 3.4. H_Z can be defined as the weighted sum of all family traits that affect earnings with schooling (and test scores, where available) controlled. "c" is the standardized coefficient of H_Z when predicting Ln Earnings,

There is a substantial effect of family background on men's earnings, net of schooling and test scores. Those traits which produce this effect (H_Z) are weakly and sometimes negatively correlated with traits which influence education (H_U) and test scores

Figure 3.3

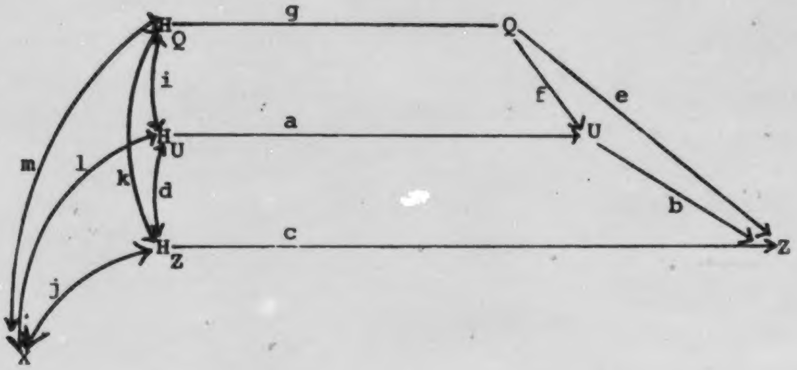


Where E_Z = all family traits that influence sons' Ln Earnings

E_U = all family traits, uncorrelated with E_Z that influence sons' education.

Solutions for:	a
Kalamazoo Brothers	.469
Talent Brothers	.455
NORC Brothers	.549
Taubman DZ Twins	.506

Figure [illegible].4


H
Q
g
Q
i
f
e
a
H
U
U
m
l
k
d
b
H
Z
c
Z
j
X

Where X = Father's Duncan score

Solutions for:	c	d	k	j
NORC Brothers	.276	-.179	NA	-.033
Kalamazoo Brothers	.348	.289	-.210	.135
Talent Brothers	.341	-.106	-.111	-.252
Taubman DZ twins	.487	.320	NA	NA

(H_Q) and with father's occupational status (X). These estimates have large standard errors, so we need not take the negative correlations too seriously.

I can think of at least two ways in which unmeasured family characteristics could affect men's earnings independently of test scores and educational attainment. **Perhaps certain** families socialize their children to develop non-cognitive skills that are largely independent of test scores and educational attainment but still valuable to employers. If we accept the notion that "like produces like," then children who have these interpersonal skills are likely to come from homes where parents also possess these skills. If employers value these interpersonal skills, then parents who have these skills should have high status jobs. But our hypothetical variable (H_Z) is uncorrelated with parental status. This suggests that H_Z does not primarily measure non-cognitive skills that employers value in parents.

Another possibility is that some families encourage their children to maximize earnings, while others encourage their children to maximize other forms of psychic income, even at the expense of earnings. If this were the case, we would expect the unmeasured family characteristics that maximize earnings net of test scores and educational attainment (i.e. parental values) to be quite different from those that maximize test scores and educational attainment. Indeed, the correlation of H_Z with H_U and H_Q might well be negative.

All the models above assume that family effects on brothers' schooling, test scores, occupations, and earnings are symmetric, and that the characteristics of one brother do not directly affect the characteristics of another brother. Other assumptions are equally plausible. For instance, parents may try to equalize their children's chances for success by devoting more resources to their less gifted children. Or younger brothers might emulate their older brothers, so that the schooling of the older brother might affect that of the younger brother. We do not know enough information to test the assumption that the characteristics of one brother directly affect those of another brother.

Conclusions

Our analyses support the following conclusions about effects of family background on men's earnings.

1. Measured parental advantages are moderately related to men's earnings. This relationship appears to have declined between 1961 and 1972. Linear and additive models adequately capture the relationship between measured background and Ln Earnings. Our best estimate is that even with perfect measurement, "demographic" background characteristics accounted for less than one-fifth of the variance in Ln Earnings for men 25-64 in the early 1970's.

2. Most of the effect of measured parental advantages is indirect. Parental characteristics exert their influence on sons' earnings by affecting sons' test scores and education. The one exception to this rule is race. Blacks earn considerably less than whites with comparable schooling and test scores.

3. The costs of being black and of growing up in the South declined between 1961 and 1972.

4. The effects of parental advantages on earnings were not generally mediated by personality or motivation variables. The one exception was that part of the effect of a father's education on his son's hourly earnings at 28 was explained by the son's occupational aspirations at 17.

5. Measured parental advantages capture only part of the influence of family background on men's earnings. We might explain as much as 30 percent of the variance in Ln Earnings in one year and as much as 42 percent of the variance in lifetime earnings if we could add the unmeasured background traits that make brothers alike in earnings to our analyses.

6. If we consider the overall effects of families on individual attainment, no one set of traits can explain sibling resemblance on test scores, schooling and earnings. The family traits that influence earnings are not a subset of those which influence test scores and schooling, nor vice versa.

7. Unmeasured parental characteristics exert considerable impact on son's earnings, independently of test scores and educational attainment. The unmeasured characteristics that create these effects appear to be almost uncorrelated with the measured advantages typically found to influence men's earnings. We cannot say for sure what these unmeasured characteristics are, or how they affect earnings.

8. Our data do not allow us to test all the assumptions used in estimating the impact of unmeasured family characteristics on a son's earnings. For instance, we cannot test the assumption that a man's characteristics do not affect his brother's characteristics.

Nor can we test the hypothesis that parents try to equalize their children's chances of success by devoting more resources to their less gifted children.

Chapter 4
Effects of Academic Ability

By James Crouse

This chapter explains what we have learned about the effects of cognitive skills on educational attainment and economic success. Men who score well on tests of these skills tend to obtain higher status jobs and earn more money than lower scoring men. This is not because they come from families that boost their earnings. Nor is it because of the personality characteristics for which we have measures. We will see that they succeed economically primarily because they are selectively encouraged to have higher aspirations and to attend school longer. But even if they do not have unusually high aspirations or get more schooling than average, men with high scores seem to be worth more to employers who hire, fire, and pay them. This may be because they are more productive or because employers have an economically irrational preference for workers with high scores and are willing to pay modest amounts to indulge this prejudice.

This chapter is divided into three sections. The first section describes the relative importance of different skills nominally taught in different kinds of high school courses, i.e., English, Social Studies, Mathematics, Physical Science, etc. In this section I describe the bivariate relationships of scores on these tests to later success. The data come from the Talent survey. The second section describes the relative importance of high school cognitive skills as against other factors in determining life chances. Here I examine bivariate and then multivariate relationships for several samples for whom we have test scores prior to school completion, i.e., Talent, Kalamazoo, Wisconsin, and others. The third section describes the importance of cognitive skills after school completion for determining

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* In addition to my colleagues on this project, I am indebted to Lee Cronbach, Arthur Hoerl, Arthur Jensen, Jon Magoon, and Victor Martuza for critical comments on an earlier draft of this chapter.

life chances. These analyses use the Veterans and [illegible] surveys.

1. Which Tests Matter?

All high schools try to teach most of their students academic subjects like English, Mathematics, and Social Studies. The Talent tests tried to measure students' mastery of skills and information commonly taught in such courses. I have no way of knowing whether the tests achieved this objective.

Table 4.1 shows correlations between thirty different tests and later success. I have divided the tests into four arbitrary categories: academic subjects, non-academic subjects, aptitude and ability, and rote memory. The classification is based on the subject matter of the tests. It is not meant to suggest that performance on the "academic subject matter" tests is independent of ability or rote memory, nor even that the "aptitude and ability" tests measure aptitude better than the other tests. The data come from a sample of 839 male Talent students who were tested in eleventh grade. I describe the sample in detail in Appendix H. Their Education, Occupation and Earnings were obtained 11 years after their high school graduation, when they were about 28 years old.

Information in academic areas correlates higher with success than information in non-academic areas, but the differences are not large. The differences disappear almost entirely when the correlations are corrected for unreliability.[1]

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[1] Computed as $r_{t^*s} = r_{ts}/r_{tt^*}^{\frac{1}{2}}$ where r_{t^*s} is the corrected correlation between test performance and success, r_{ts} is the observed correlation, and r_{tt^*} is the reliability in Table 4.1. I did not attempt to correct the measures of success for unreliability, since such corrections would not affect my conclusions in this section.

Table 4.1

Correlations of Test Scores and Outcomes in Talent Sample of 839 Men

Type of Test	Test (number of Items)	Talent Designation	Estimated Reliability[a]	Observed Correlations (1) Years of Education	(2) Occupation	(3) Hourly Earnings	(4) Ln Hourly Earnings	Correlations of test (but not Outcome) Corrected for Unreliability (5) Years of Education	(6) Occupation	(7) Hourly Earnings	(8) Ln Hourly Earnings
Academic Subjects											
	English (n=113)	R-230	.921	.471	.423	.164	.173	.491	.441	.171	.180
	Literature (n=24)	R-103	.817	.510	.439	.162	.162	.564	.486	.179	.179
	Social Studies (n=24)	R-105	.835	.499	.436	.176	.172	.546	.477	.193	.188
	Mathematics Information (n=23)	R-106	.892	.550	.495	.219	.206	.582	.524	.232	.218
	Arithmetic Computation (n=72)	R-410	NA	.338	.316	.166	.164	NA	NA	NA	NA
	Arithmetic Reasoning (n=16)	R-311	.779	.425	.338	.148	.152	.482	.383	.168	.172
	Introductory Mathematics (n=24)	R-312	.893	.516	.421	.191	.189	.546	.446	.202	.200
	Advanced Mathematics (n=14)	R-333	.773	.474	.360	.176	.148	.539	.409	.200	.168
	Physical Science (n=18)	R-107	.848	.454	.364	.131	.123	.493	.395	.142	.134

Table 4.1 Continued
Correlations of Test Scores and Outcomes in Talent Sample of 839 Men

Type of Test	Test (N of Items)	Talent Designation	Estimated Reliability[a]	Observed Correlations				Correlations of Test (but not Outcome) Corrected for Unreliability			
				(1) Years of Education	(2) Occupation	(3) Hourly Earnings	(4) Ln Hourly Earnings	(5) Years of Education	(6) Occupation	(7) Hourly Earnings	(8) Ln Hourly Earnings
	Biological Science (n=11)	R-108	.665	.381	.315	.107	.095	.467	.386	.131	.116
	Mean of the Ten Correlations[b]			.462	.391	.164	.158	.523	.439	.180	.173
Non-Academic Subjects											
	Music (n=13)	R-104	.749	.459	.399	.180	.183	.530	.461	.208	.211
	Art (n=12)	R-131	.678	.381	.357	.182	.185	.463	.434	.221	.225
	Home Economics (n=21)	R-114	.512	.217	.180	.125	.116	.303	.252	.175	.162
	Law (n=9)	R-132	.574	.377	.314	.107	.116	.498	.414	.141	.153
	Health (n=9)	R-133	.625	.367	.361	.101	.103	.464	.457	.128	.130
	Architecture (n=6)	R-135	.415	.325	.309	.078	.071	.504	.480	.121	.110
	Photography (n=3)	R-148	.240	.285	.199	.107	.114	.582	.406	.218	.233
	Theater (n=8)	R-150	.598	.369	.320	.148	.152	.477	.414	.191	.197

Table 4.1 Continued
Correlations of Test Scores and Outcomes in Talent Sample of 839 Men

Type of Test	Test (N of Items)	Talent Designation	Estimated Reliability[a]	Observed Correlations				Correlations of Test (but not Outcome) Corrected for Unreliability			
				(1) Years of Education	(2) Occupation	(3) Hourly Earnings	(4) Ln Hourly Earnings	(5) Years of Education	(6) Occupation	(7) Hourly Earnings	(8) Ln Hourly Earnings
	Farming (n=12)	R-113	.635	.160	.143	.060	.055	.201	.179	.075	.069
	Mean of the Nine Correlations[b]			.326	.287	.121	.122	.447	.389	.164	.166
Aptitude and Ability Tests											
	Reading Comprehension (n =48)	R-250	.919	.489	.405	.178	.176	.510	.422	.186	.184
	Vocabulary (n=30)	R-172	.849	.482	.428	.184	.191	.523	.465	.200	.207
	Creativity (n=20)	R-260	.799	.352	.311	.130	.117	.394	.348	.145	.131
	Mechanical Reasoning (n=15)	R-270	.852	.256	.247	.122	.111	.277	.268	.132	.120

Table 4.1 Continued
Correlations of Test Scores and Outcomes in Talent Sample of 839 Men

ype f est	Test (N of Items)	Talent Designation	Estimated Reliability[a]	Observed Correlations				Correlations of Test (but not Outcome) Corrected for Unreliability			
				(1) Years of Education	(2) Occupation	(3) Hourly Earnings	(4) Ln Hourly Earnings	(5) Years of Education	(6) Occupation	(7) Hourly Earnings	(8) Ln Hourly Earnings
	Abstract Reasoning (n=15)	R-290	.744	.361	.354	.153	.162	.419	.410	.177	.188
	Visualization (n=16)	R-282	.796	.268	.256	.125	.119	.300	.287	.140	.133
	Table Reading (n=72)	R-420	NA	.003	.054	.087	.109	NA	NA	NA	NA
	Clerical Checking (n=74)	R-430	NA	.051	.054	.092	.107	NA	NA	NA	NA
	Object Inspection (n=40)	R-440	NA	-.006	.014	.023	.042	NA	NA	NA	NA
	Mean of the Nine Correlations[b]			.251	.236	.122	.126	.404	.367	.163	.161
easures f Rote Memory											
	Memory for Sentences (n=16)	R-211	.613[c]	.095	.071	.040	.037	.121	.091	.051	.047
	Memory for Words (n=24)	R-212	.824[c]	.282	.228	.103	.107	.311	.251	.113	.118
	Mean of the Two Correlations[b]			.189	.150	.072	.072	.216	.171	.082	.083

NOTES FOR TABLE 4.1

a. Split-half reliabilities reported in Table 4-7 of Shaycroft (1967).

b. Since all correlations were less than 0.60, the means were calculated arithmetically rather than with the z transformation.

c. Kuder-Richardson reliabilities (KR-21) reported in Table 2-5 of Flanagan, et al. (1964). KR-21 sets a lower bound for reliability so its use in correcting correlations for unreliability could result in some upward bias in the corrected correlations.

As we will see, students who are informed in academic areas tend to be informed in non-academic ones as well. But the correlation between academic and non-academic performance is far from perfect.

The aptitude and ability tests differ considerably in their prediction of success. The most important of these tests correlate as highly with success as the most important academic subjects. But skills like Table Reading, Clerical Checking, and Object Inspection, in which schools seem to have little interest, have almost no correlation with success.

Rote memory does not predict success at all well, even after correction for attenuation. Tests like Memory for Sentences and Memory for Words have sometimes been considered measures of an individual's learning quotient. If this is correct, learning quotient is of little economic importance. The small correlations of rote memory with educational attainment are also hard to reconcile with critiques of schooling that claim schools only reward rote memory.

Finally, tests which predict educational attainment also predict occupation and earnings, while tests that do not predict education accurately do not predict occupation and earnings. If we rank each test by its ability to predict each of the four measures of success in Table 1, the correlations between the pairs of rankings average 0.873 for the 30 tests. This is because, as we will see later, the skills measured by those tests affect economic success largely by influencing education.[2]

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[2] The correlation between the 30 tests' ability to predict education and their ability to predict occupation is 0.982. The correlation is 0.842 for education and earnings, 0.779 for education and ln earnings, 0.856 for occupation and earnings, 0.804 for occupation and ln earnings, and 0.975 for earnings and ln earnings. These values differ partly because the correlations of most tests with education are higher than with occupation, and higher with occupation than with the earnings measures. The sampling errors are larger relative to the mean when the correlations are low, making the rank-order of the tests less reliable for such outcomes. The values also differ partly because the correlations between outcomes differ. Education is more highly correlated with occupation than earnings. One therefore expects a higher correlation between the education and occupation columns than between the education and earnings columns. The high correlation between the two earnings columns is because they are essentially the same outcome.

Importance of Different Abilities

The tests in Table 4.1 measure diverse traits. In order to get a better sense of their relative importance, I factor analyzed the tests covering academic subjects and used the first principal component as an index of academic ability. I did the same thing for tests that had a "verbal" label, a "quantitative" label, and a label stressing rote memory. Table 4.2 shows that the first factor accounts for between 63.0 and 76.4 percent of the variance in each of the four groups of tests. Part of the variance unexplained by the factors is due to measurement error. If, for example, the reliability of a test is 0.80, then a factor cannot explain more than 80 percent of this test's variance. In order to estimate the percent of stable non-error variance explained by the factors, I corrected the correlations for test unreliability and recomputed each factor analysis.[3]

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[3] I corrected the correlations by assuming $r_{t_1^* t_2^*} = r_{t_1 t_2} / \{r_{t_1 t_1^*} r_{t_2 t_2^*}\}^{1/2}$ where $r_{t_1^* t_2^*}$ is the corrected correlation between two tests, $r_{t_1 t_2}$ is the observed correlation, and $r_{t_1 t_1^*}$ and $r_{t_2 t_2^*}$ are the reliabilities for the two tests. The reliabilities were taken from Table 4.1. I corrected the diagonal elements of each correlation matrix by inserting the test reliabilities. This obviously introduces some random error, since the reliabilities were not estimated from the present sample.

Table 4.2

First Principal Components of Talent's Academic Achievement, Verbal, Quantitative, and Rote Memory Tests

	Factor Loadings							
	Observed				Corrected for Unreliability			
	Academic Ability	Verbal Ability	Quantitative Ability	Rote Memory	Academic Ability	Verbal Ability	Quantitative Ability	Rote Memory
English	.821	.850			.853	.877		
Literature	.783	.855			.843	.895		
Social Studies	.822	.882			.879	.920		
Mathematics Information	.891		.911		.933		.953	
Arithmetic Computation[1]	.638		.652		.641		.660	
Arithmetic Reasoning	.789		.807		.857		.864	
Introductory Mathematics	.870		.903		.910		.943	
Advanced Mathematics	.761		.809		.827		.864	
Physical Science	.833		.828		.890		.879	
Biological Science	.694		.670		.797		.749	
Reading Comprehension		.876				.905		
Vocabulary		.906				.942		
Memory for Sentences				.805				.659
Memory for Words				.805				.846
Percent of Variance Explained by First Principal Component	63.0	76.4	64.4	64.9	85.1	95.0	86.5	80.0

Note: Principal components extracted only for tests listed under each heading. Resulting components are neither independent nor orthogonal.

1. Reliability estimates not available.

This substantially increases the percent of variance explained by each factor. Table 4.2 shows that after these corrections each of the four factors explains 80to 95 percent of the stable non-error variance . The four factors therefore seem to capture the skills measured by these 30 tests quite adequately.

Table 4.3 shows correlations between the factors and four measures of success. Academic ability has the highest correlation with the dependent variables. Verbal and quantitative abilities are next in importance. There is no striking evidence that one is more important than the other. Rote memory tests are again the least important predictors of success.

Talent also computed a priori academic, verbal and quantative composites. Table 4.3 presents correlations between these three composites and success. The results parallel those found with the factors. The academic composite predicts success better than the verbal and quantitative composites.

The verbal and quantitative composites do not differ consistently. Thus the Talent data do not support Taubman and Wales' (1973, 1974) finding that quantitative skills have a greater relationship with economic success than verbal skills. 4/

4/ Taubman and Wales (1974, 1973) have suggested that mathematical ability is a more important determinant of income than verbal ability. However, their verbal factor included substantial weightings of tests for mechanical principles, spacial orientation, and two-hand coordination, which may have reduced its ability to predict success.

Table 4.3
Correlations of Factors, Composites, and Selected Tests With Outcomes and With Each Other: Talent Sample of 839 Males

Factor or Composite	Measures of Success				Factors			
	Years of Education	Occupation	Hourly Earnings	Ln Hourly Earnings	Academic	Verbal	Quantitative	Rote Memory
Factors								
Academic Ability[1]	.585	.490	.211	.204	1.000			
Verbal Ability[2]	.560	.487	.200	.202	.924	1.000		
Quantitative Ability[3]	.564	.460	.205	.194	.980	.841	1.000	
Rote Memory[4]	.234	.185	.089	.090	.417	.412	.397	1.000
Composites								
Academic Ability[5]	.561	.474	.203	.203	.953	.928	.925	.423
Verbal Ability[6]	.531	.470	.182	.188	.905	.959	.826	.442
Quantitative Ability[7]	.554	.445	.208	.198	.932	.772	.966	.356
Selected Tests								
English	.471	.423	.164	.173	.823	.850	.752	.421
Social Studies	.499	.436	.176	.172	.821	.882	.730	.353
Introductory Mathematics	.516	.421	.191	.189	.870	.711	.903	.338
Table Reading	.003	.054	.087	.109	.085	.063	.088	.067
Clerical Checking	.051	.054	.092	.107	.040	.011	.047	.048
Reading Comprehension	.489	.405	.178	.176	.780	.875	.731	.341
Vocabulary	.482	.428	.184	.191	.831	.906	.779	.370
Memory for Sentences	.095	.071	.040	.037	.209	.209	.206	.805
All 30 Tests[8]	.609	.520	.219	.226	---	----	---	---

Table 4.3 Continued
Correlations of Factors, Composites, and Selected Tests With Outcomes and With Each Other: Talent Sample of 839 Males

Factor or Composite	Composite			Selected Tests							
	Academic	Verbal	Quantitative	English	Social Studies	High School Mathematics	Table Reading	Clerical Checking	Reading Comprehension	Vocabulary	Memory for Sentences
Composite											
Academic Ability	1.000										
Verbal Ability	.933	1.000									
Quantitative Ability	.904	.770	1.000								
Selected Tests											
English	.893	.949	.715	1.000							
Social Studies	.754	.773	.656	.662	1.000						
Introductory Mathematics	.855	.713	.953	.669	.609	1.000					
Table Reading	.085	.084	.071	.095	.070	.080	1.000				
Clerical Checking	.022	.034	.034	.031	.003	.046	.458	1.000			
Reading Comprehension	.870	.789	.678	.713	.704	.623	.039	-.017	1.000		
Vocabulary	.828	.875	.709	.723	.753	.643	.049	-.007	.748	1.000	
Memory for Sentences	.231	.239	.174	.235	.176	.169	.073	.007	.192	.199	1.000

Notes for Table 4.3

1. First principal component of the ten tests of academic subjects in Table 1.
2. First principal component of the English, Literature, Social Studies, Reading Comprehension, and Vocabulary tests.
3. First principal component of the Mathematics Information, Arithmetic Computation, High School Mathematics, Advanced Mathematics, Physical Science, and Biological Science tests.
4. First principal component of Memory for Sentences and Memory for Words tests.
5. Talent's Academic Composite, C-002, a weighted sum of Mathematics Information, Vocabulary, English, Reading Comprehension, Creativity, Abstract Reasoning, Arithmetic Reasoning, and High School Mathematics.
6. Talents Verbal Composite, C-003, a weighted sum of Literature, Vocabulary, and English.
7. Talent's Quantitative Aptitude Composite, C-004, a weighted sum of Mathematics Information, Arithmetic Reasoning, High School Mathematics, and Advanced Mathematics.
8. Multiple correlation of each measure of success with all 30 tests, corrected for degrees of freedom, i.e. $(\bar{R}^2)$.

The academic, verbal, and quantative factors and composites correlate very highly with one another. Rote memory correlates poorly with these abilities and with the dependent variables. In general, tests that do not correlate well with the factors and the composites and do/not correlate well with each other, do not predict economic success very well. They also measure traits in which schools take little formal interest. Table/4.3 illustrates this for eight selected tests. The other tests in the Talent battery .. follow the same pattern.

I conclude that general academic ability and general verbal and quantitative ability matter more for educational and economic success than other kinds of information or cognitive skills. Academic, verbal and quantitative skills are highly correlated. Common sense suggests, however, that they are quite different. Having a large vocabulary is different from being able to solve quadratic equations. They must therefore correlate because they have common causes. They also seem to have similar effects at least on economic success. For studies of stratification, then, they are interchangeable.

In order to determine the overall importance of these factors and composites, I regressed the four dependent variables separately on all 30 of the tests shown in Table 1. The 30 tests explained 37.1 percent of the variance in Education, 27.0 percent in Occupation, 4.8 percent in Hourly Earnings, and 5.1 percent in Ln Hourly Earnings.5/

5/ These percentages are corrected for degrees of freedom.

This was not significantly more than the academic factor, which explained 34.2 percent of the variance in Education, 24.0 percent of the variance in Occupation, 4.5 percent of the variance in Hourly Earnings, and 4.2 percent of the variance in Ln Hourly Earnings. The a priori composites did not do quite as well and the individual tests often did considerably worse. This suggests that the tests typically used in survey research somewhat underestimate the relationship between test performance and adult status.

Nonlinearities

The results so far could be misleading if test scores were not linearly related to success. Table/4.4 shows regressions of Education, Occupation, Hourly Earnings, and Ln Hourly Earnings on the 30 tests from the Talent survey. I standardized each test to a mean of 100 and / standard deviation of 15 using the mean and standard deviation of the Talent sample of 839 men for these regressions. The sign of the Test Score^2 coefficient is positive in most of the Education equations, and is often significant. This is what one would expect if the full population regressions were linear, since this sample excludes those with less than 11 years of school. The sign of the Test Score^2 coefficient is as likely to be positive as negative in the Occupation equations, and it is statistically significant in only a few cases. The Test Score^2 coefficient has a negative sign in the majority of cases for the Hourly Earnings and Ln Hourly Earnings equations, but it is significant for only a few of the tests. The most striking feature of these results is that the addition of the Test Score^2 term boosts R^2 by less than 0.02, except

Table 4.4

Regressions of Respondent's Characteristics on Test Scores for 839 Talent Males: Sign of Test Score^2 (TS^2) and R^2 Increment (ΔR^2) Due to Test Score^2

Test	Education		Occupation		Hourly Earnings		Ln of Hourly Earnings	
	TS^2	ΔR^2	TS^2	ΔR^2	TS^2	ΔR^2	TS^2	ΔR^2
Academic Subjects								
English	+	.01667	[+]	.00093	[-]	.00009	[-]	.00004
Literature	+	.01080	[+]	.00042	[-]	.00076	[-]	.00129
Social Studies	+	.01040	[+]	.00090	[-]	.00116	[-]	.00125
Mathematics Information	[+]	.00006	[-]	.00315	[-]	.00003	[-]	.00079
Arithmetic Computation	[+]	.00087	[+]	.00010	[-]	.00001	[-]	.00003
Arithmetic Reasoning	+	.00728	[+]	.00138	[+]	.00007	[-]	.00001
Introductory Mathematics	[+]	.00102	[-]	.00263	[-]	.00116	[-]	.00413
Advanced Mathematics	[+]	.00026	[-]	.00048	[-]	.00092	[-]	.00252
Physical Science	+	00622	[-]	.00028	[+]	.00020	[+]	.00000
Biological Science	[+]	.00380	[+]	.00182	[+]	.00001	[+]	.00001
Non-Academic Subjects								
Music	+	.00639	[+]	.00001	[+]	.00001	[-]	.00011
Art	[-]	.00001	[-]	.00128	[-]	.00174	[-]	.00216
Home Economics	[+]	.00054	[-]	.00001	[-]	.00293	-	.00554
Law	[+]	.00152	[+]	.00142	-	.00738	[-]	.00400
Health	[+]	.00206	[+]	.00014	[-]	.00116	[-]	.00091
Architecture	[+]	.00400	[+]	.00189	[-]	.00005	[+]	.00005
Photography	[+]	.00085	[-]	.00294	[-]	.00046	[-]	.00065
Theater	+	.00625	[+]	.00111	[-]	.00079	[-]	.00106
Farming	[+]	.00107	[-]	.00049	-	.00608	[-]	.00462

Table 4.4 Continued
Regressions of Respondent's Characteristics on Test Scores for 839 Talent Males: Sign of Test Score^2 (TS^2) and R^2 Increment (R^2) Due to Test Score^2

Test	Education		Occupation		Hourly Earnings		Ln of Hourly Earnings	
	TS^2	ΔR^2	TS^2	ΔR^2	TS^2	ΔR^2	TS^2	ΔR^2
Aptitude and Ability Tests								
Reading Comprehension	+	.01514	[+]	.00204	[-]	.00033	[-]	.00025
Vocabulary	+	.02013	+	.00469	[-]	.00288	[-]	.00280
Creativity	[+]	.00065	[-]	.00026	[-]	.00000	[-]	.00007
Mechanical Reasoning	[+]	.00409	[+]	.00001	[-]	.00128	[-]	.00106
Abstract Reasoning	+	.00512	[-]	.00075	-	.00460	-	.00541
Visualization	[+]	.00284	[+]	.00029	[+]	.00021	[+]	.00005
Table Reading	-	.03227	-	.03000	-	.00795	-	.00735
Clerical Checking	-	.05952	-	.03204	-	.00801	-	.00751
Object Inspection	-	.01050	-	.00885	[-]	.00225	[-]	.00195
Rote Memory								
Sentences	-	.00719	-	.00730	[+]	.00001	[-]	.00012
Words	[-]	.00400	[-]	.00266	[-]	.00039	[-]	.00099

Note: Coefficients less than twice their standard error are in brackets.

for two of the 30 tests, Table Reading and Clerical Checking. Unlike all the other tests, these two tests consistently have significant negative Test Score^2 terms. We will see shortly that they also have distributional properties which differ from all the other tests. Therefore I do not believe results for these two tests should be counted too heavily.

Table 4.4 shows that non-linear test score effects are often significant, especially when the sample is restricted on the dependent variable. This is true also in the Veterans, Kalamazoo, and PSID surveys. But Test Score^2 also has different signs with different tests and outcomes and does not add much to the explained variance.

I therefore conclude that non-linear effects are not very important in discussions of the effects of cognitive skills on measures of socioeconomic success.

The conclusion that non-linear effects are small and inconsistent could be wrong if the ranges of the test score distributions are truncated and mask non-linearities at high or low test score values. The absence of men with fewer than 11 years of education in the Talent sample means that we know little about the effects of test scores at very low test score values. Their absence also leads me to expect some positive skew in the Talent distributions. Table 4.5 shows the skewness and kurtosis of the 30 Talent test score distributions. Columns 1 and 2 show the values for scores standardized to a mean of 100 and SD of 15 using the

Table 4.5

Skewness and Kurtosis of Observed Test Scores and Test Scores Scaled Under Different Assumptions: Talent Sample of 839 Males

Test	Test Score		Education		Hourly Earnings	
	Skewness	Kurtosis	Skewness	Kurtosis	Skewness	Kurtosis
Academic Subjects						
English	- .753	1.510	.320	.337	-1.054	2.609
Literature	- .141	- .481	.559	- .294	- .775	.283
Social Studies	- .657	- .255	- .044	-1.088	-1.317	1.539
Mathematics Information	.146	-1.100	.185	-1.083	.067	-1.126
Arithmetic Computation	- .318	.851	.102	.555	- .428	1.007
Arithmetic Reasoning	- .233	- .698	.373	- .847	- .050	- .829
Introductory Mathematics	.122	- .882	.314	- .835	- .434	- .567
Advanced Mathematics	.663	- .034	.789	.219	.056	- .800
Physical Science	- .118	- .849	.407	- .803	.215	- .889
Biological Science	- .343	- .500	.184	- .810	- .258	- .593
Non-Academic Subjects						
Music	.029	- .741	.597	- .406	.097	- .734
Art	- .087	- .459	- .120	- .451	- .889	.364
Home Economics	.196	.060	.658	.727	-1.236	1.564
Law	- .150	- .318	.240	- .359	-2.247	5.785
Health	- .594	- .087	- .157	- .667	-1.821	3.581
Architecture	.170	- .560	.845	.241	- .102	- .613
Photography	- .061	- .337	.334	- .129	- .740	.114
Theater	- .057	- .532	.660	- .230	- .697	.129
Farming	- .843	.829	- .059	- .497	-4.115	22.387

Table 4.5 Continued
Skewness and Kurtosis of Observed Test Scores and Test Scores Scaled Under Different Assumptions: Talent Sample of 839 Males

Test	Test Score		Education		Hourly Earnings	
	Skewness	Kurtosis	Skewness	Kurtosis	Skewness	Kurtosis
Aptitude and Ability Tests						
Reading Comprehension	- .716	- .058	.015	-1.138	-1.094	.980
Vocabulary	- .534	- .364	.349	- .832	-1.548	2.038
Creativity	- .075	- .517	.189	- .451	- .152	- .508
Mechanical Reasoning	- .626	.032	.171	- .874	-1.861	4.401
Abstract Reasoning	- .652	.352	.099	- .405	-2.349	6.378
Visualization	- .328	- .503	.303	- .808	.067	- .805
Table Reading	3.506	17.981	-3.001	19.474	.255	.228
Clerical Checking	1.093	.604	-1.424	5.191	-1.217	3.893
Object Inspection	.421	.009	-2.310	7.312	2.690	11.935
Rote Memory						
Sentences	- .124	- .211	-2.459	7.699	.143	- .282
Words	.545	- .344	- .205	- .620	- .132	- .647

Table 4.5

Skewness and Kurtosis of Observed Test Scores and Test Scores Scaled Under Different Assumptions: Talent Sample of 839 Males

Test	Test Score		Education		Hourly Earnings	
	Skewness	Kurtosis	Skewness	Kurtosis	Skewness	Kurtosis
Academic Subjects						
English	- .753	1.510	.320	.337	-1.054	2.609
Literature	- .141	- .481	.559	- .294	- .775	.283
Social Studies	- .657	- .255	- .044	-1.088	-1.317	1.539
Mathematics Information	.146	-1.100	.185	-1.083	.067	-1.126
Arithmetic Computation	- .318	.851	.102	.555	- .428	1.007
Arithmetic Reasoning	- .233	- .698	.373	- .847	- .050	- .829
Introductory Mathematics	.122	- .882	.314	- .835	- .434	- .567
Advanced Mathematics	.663	- .034	.789	.219	.056	- .800
Physical Science	- .118	- .849	.407	- .803	.215	- .889
Biological Science	- .343	- .500	.184	- .810	- .258	- .593
Non-Academic Subjects						
Music	.029	- .741	.597	- .406	.097	- .734
Art	- .087	- .459	- .120	- .451	- .889	.364
Home Economics	.196	.060	.658	.727	-1.236	1.564
Law	- .150	- .318	.240	- .359	-2.247	5.785
Health	- .594	- .087	- .157	- .667	-1.821	3.581
Architecture	.170	- .560	.845	.241	- .102	- .613
Photography	- .061	- .337	.334	- .129	- .740	.114
Theater	- .057	- .532	.660	- .230	- .697	.129
Farming	- .843	.829	- .059	- .497	-4.115	22.387

Table 4.5 Continued
Skewness and Kurtosis of Observed Test Scores and Test Scores Scaled Under Different Assumptions: Talent Sample of 839 Males

Test	Test Score		Education		Hourly Earnings	
	Skewness	Kurtosis	Skewness	Kurtosis	Skewness	Kurtosis
Aptitude and Ability Tests						
Reading Comprehension	- .716	- .058	.015	-1.138	-1.094	.980
Vocabulary	- .534	- .364	.349	- .832	-1.548	2.038
Creativity	- .075	- .517	.189	- .451	- .152	- .508
Mechanical Reasoning	- .626	.032	.171	- .874	-1.861	4.401
Abstract Reasoning	- .652	.352	.099	- .405	-2.349	6.378
Visualization	- .328	- .503	.303	- .808	.067	- .805
Table Reading	3.506	17.981	-3.001	19.474	.255	.228
Clerical Checking	1.093	.604	-1.424	5.191	-1.217	3.893
Object Inspection	.421	.009	-2.310	7.312	2.690	11.935
Rote Memory						
Sentences	- .124	- .211	-2.459	7.699	.143	- .282
Words	.545	- .344	- .205	- .620	- .132	- .647

Talent sample of 839 men. About two-thirds of the tests show negative skewness and kurtosis. If the Talent sample came from a normal population, we would expect sample values of skewness to be approximately normally distributed with a mean of 0 and a SD of $\sqrt{6/839} = 0.0846$. Sample values of kurtosis would be normally distributed with a mean of 0 and SD of $\sqrt{24/839} = 0.169$. Many of the Talent tests thus have distributions which deviate significantly from normality. Nevertheless, the degree of non-normality does not seem large. Skewness is greatest for Table Reading and Clerical Checking.

The fact that two-thirds of the Talent tests have negatively skewed distributions suggests that ceiling effects may have truncated the upper end of some distributions to produce the negative skew, even though poorly educated men were underrepresented in the distributions. Very few individuals got every item right, however. This suggests that the ceiling effects were not produced by ceilings on the number of items students could possibly get correct. Rather, the tests must have contained enough very difficult items to impose an artificial ceiling above which even the cleverest students seldom rose. Jencks and Brown reached a similar conclusion from their examination of longitudinal changes between ninth and twelfth grade for the Talent tests. Because the range of all distributions is restricted at the lower end, and some may be restricted at the upper end, the conclusion that nonlinearities are small must be tentative.

The distributional properties of the Talent tests just described could also be distorted if the tests were not scaled properly. I rescaled each test to have a maximum linear relationship with two outcomes: education and earnings. I did this by regressing Education on Test Score and Test Score^2 for each test. I then used the coefficient for the Test Score term and the Test Score^2 term to get a predicted Education value for each individual. This was his new "test score." Columns 3 and 4 in Table 4.5 describe the distributions of these scores. Most of the skewness and kurtosis values are still significant statistically, but they are still relatively small. They are also inconsistently positive and negative compared to the original scaling.

Columns 5 and 6 show the distributions when I rescaled the original test scores to maximize their linear relationship with Hourly Earnings. This rescaling also produced no consistent or interesting deviations from normality.

I conclude that the deviations from normality are inconsistent for the original scaling and the two rescalings I tried. This does not guarantee that the original scaling is correct. Since the original distributions underrepresent low ability individuals and may be restricted at the upper end by ceiling effects, Table 4.5 does not tell us much about the full range of ability. But the results do suggest that my scaling did not seriously distort my results for this sample.

These findings lead me to several conclusions:

First, academic ability seems to be largely but not entirely one - dimensional, at least for the purpose of predicting stratification outcomes. Academic ability correlates highly with verbal and quantitative ability, and it predicts success better than other skills which we have measured. Other tests seem to predict success only because they correlate well with these factors and composites. This does not mean that all academic skills are the same, or that they will correlate equally well with all outcomes that one might conceivably measure. It means only that their effects on socio-economic status are similar. This is probably because they share common causes.

Second, the effects of academic ability are captured better by the factors and composites I used than by most individual tests. This suggests that tests which are short or cover a narrow domain typically underestimate the true relationship between the test performance and adult status.

Third, non-linear effects of test scores do not seem very important, although they could be more important in samples with a wider range of ability.

Finally, the observed distributions did not change greatly or consistently when I rescaled the tests to make their effects linear. Thus, I doubt that the relationships I described are seriously distorted because of the scales I used.

In the analyses that follow I will use Talent's a priori Academic Ability Composite to measure academic ability in high school in the Talent sample. Table 4.3 suggests that it would have been better to use the Principal Component of the same name. But this was not available when I initiated the analyses that follow. Since the two measures correlate 0.95, the difference is minor. I will compare Talent Academic Composite with tests from other surveys for which I have data. Table 4.6 describes these surveys and some of their characteristics.

2. Effects of Early Ability

This section looks at the effects of academic competence prior to high school completion. I will

Table 4.6

Some Characteristics of Selected Longitudinal Surveys

	Talent Representative	Talent 23 Year Old Twins	Talent 29 Year Old Brothers	Wisconsin
References	Crouse, App. H	Jencks and Brown (1977)		Sewell and Hauser (1975)
Survey Organization	Talent	Talent	Talent	Wisconsin State Departments, Social Security Administration
Initial Survey Year	1960	1960	1960	1957
Initial Age (Age for Test Score Data)	11th Grade	9th - 12th Grade	11th - 12th Grade	11th Grade
Final Survey Year	1972	1965-8	1972	1967
Final Age	28-29	22-23	28-29	24-28
Test	Academic Composite	Academic Composite	Academic Composite	Henmon-Nelson
Year of Test Score Data	1960	1960	1960	1956
Sample Size	839	332	198[4]	1789
Sample Restrictions	Only men who reached 11th grade [1]	Only twins who reached 9th grade and had a twin in the same school[3]	Only men who reached 11th grade and had a brother in 11th or 12th grade	Only men who reached 12th grade[5]

Table 4.6 Continued

Some Characteristics of Selected Longitudinal Surveys

	EEO	Kalamazoo	Mälmo	Veterans	PSID
References	Alexander, Eckland, and Griffin (1975)	Olneck, Appendix I	Fägerlind (1975)	Jencks, Appendix G	Mueser, Appendix D
Survey Organization	ETS, IRSS	Olneck	National Archives of Sweden	CPS	SRC
Initial Survey Year	1955	1928-52	1938	1964	1971[3]
Initial Age (Age for Test Score Data)	10th Grade	6th Grade	3rd Grade	20-26	25-64[2]
Final Survey Year	1970	1974	1971	1964	1972[3]
Final Age	31	35-57	42	30-34	25-64[2]
Test	**Academic Aptitude**[9]	**Terman/Otis**	**Modified Halgran Group Intelligence Test**	**AFQT**	**Sentence Completion Test**
Year of Test Score Data	1955	1928-52	1938	**1947-1962**	1972
Sample Size	538	692	707	803	1774
Sample Restrictions	Only students who reached 10th grade and attended predominantly white institutions	Only men who attended Kalamazoo public school and who had a brother in these schools	Only men who were in 3rd grade in Mälmö, Sweden in 1938	Only Veterans[6]	Only men who were heads of households[7]

Notes for Table 4.6

1. Talent complete data sample. The initial Talent Survey included about 400,000 students. See Crouse, Appendix H.
2. Age in 1972.
3. The PSID is a longitudinal survey in which the first wave of data was collected in 1968. Our sample comes entirely from the 1971 and 1972 waves.
4. Number of individuals with data on background, test scores, education, occupation, and earnings.
5. Male Wisconsin high school graduates in 1957 with nonfarm background, not enrolled in school, employed in civilian labor force in 1964, nonzero earnings 1965-67, and having complete data. See Sewell and Hauser (1975), Ch. 4.
6. Veterans with complete data. See Jencks, Appendix G.
7. PSID complete data sample. See Mueser, Appendix C.
8. Data on Academic Composite and Education. Includes 180 females.
9. A 20 item academic aptitude test giving approximately equal weight to vocabulary and arithmetic reasoning. The test is described by Stice and Ekstrom (1964)

try to determine how much of the correlation between test scores and adult success is due to their both being affected by family. I will also try to show in some detail how ability has its effects. I will examine the effects of early test scores on Education, Occupation, and Earnings.

Ability and Education

Historical Changes

The mean level of educational attainment has increased steadily in the United States since the turn of the century. Very little is known, however, about changes in the dependency of educational attainment on men's test scores in elementary or secondary school. This is somewhat surprising in light of claims that schools are becoming more meritocratic, less meritocratic, or not changing. The topic has never been investigated in a national survey, but some local data exist .

The Kalamazoo, Michigan public school system has preserved the results of its ability testing program since 1928. Olneck has described these data in detail in Appendix I. The correlation between sixth grade test scores and educational attainment does not appear to have changed much since the late 1920's. I examined correlations between sixth grade Terman/or Otis scores and Education in four cohorts of Kalamazoo men who were born in 1919-1923, 1923-1928, 1929-1933, and 1934-1938. There were 219, 228, 199 and 150 men in the four cohorts. The correlations were 0.555, 0.472, 0.616, 0.541, respectively. These changes are not large. Nor are they in any consistent direction.[1]

The Education standard deviation has decreased since 1919 in the U.S. It was smaller in Kalamazoo than in the U.S. in 1919, but it has not fallen much in Kalamazoo since 1919. Thus, educational attainment has become more homogeneous since 1919 in the U.S., but has been relatively homogeneous all along in Kalamazoo. When I correct the correlations for the restricted range of Education in Kalamazoo they become 0.595, 0.526, 0.623, and 0.552 for the four cohorts. The changes over time still remain small and have no consistent trends.[6]

Taubman and Wales (1972) argue that the effect of test scores on whether high school graduates entered college was greater in the 1950's than in earlier decades. This could imply that educators at all levels were placing more emphasis on test scores. Alternatively, test scores' increasing effect on high school graduates' chances of attending college could have been offset by a decline in tests' effects on whether students finished high school and in their effect on whether college entrants graduated. Taken in isolation, then, Taubman and Wales' data are inconclusive.[7]

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[6] The standard deviations of education are 2.54, 2.55, 3.05, and 2.77 for the 1919-1923, 1923-1928, 1929-1933, and 1934-1938 cohorts. Roughly corresponding Census values estimated by Jencks, et al. are 3.30, 3.21, 3.21, and 2.92. The test score standard deviations are 14.36, 16.69, 17.01, and 13.66. The unstandardized coefficients are 0.0983, 0.0720, 0.1098, and 0.1096. The unstandardized coefficients rise slightly among younger men, although the differences are not significant. The education standard deviations for the older Kalamazoo cohorts are more restricted compared to the Census. This implies that the Kalamazoo data may underestimate the national correlation more for the older cohorts than for younger cohorts. I have no evidence on the representativeness of the Kalamazoo test score standard deviations. I corrected the correlations using the census standard deviations, assuming that $r_{TE} = [\,r_{te}(s_E/s_e)\,] / [\,1 - r_{te}^2 + r_{te}^2(s_E/s_e)^2\,]^{\frac{1}{2}}$ (McNemar, 1962: 144) where r_{TE} and r_{te} are the corrected and observed correlations, and s_E and s_e are the census and Kalamazoo standard deviations. The corrected correlations are 0.595, 0.526, 0.623, and 0.552. This suggests that there was little change in the relationship between test scores and education for men born between 1919-1938.

[7] Taubman and Wales base their findings on slope coefficients and average IQ for attenders and non-attenders. This tells us little about correlations, since variances are unknown. It is also worth noting that Taubman and Wales based their conclusions on comparisons among eight samples, some of which were local. The samples took different tests. There is no way of knowing from their report how comparable the different cohorts and tests were.

As a further check on whether the test score-education correlation has changed, I compared Benson's sample of Minneapolis sixth graders who took the Haggerty Intelligence Examination in 1923 with the Talent sample which was tested in 1960 and typically completed their education during the 1960's. Benson reports a correlation of 0.57, which Duncan, Featherman, and Duncan (1972) reestimate at 0.542. The Talent correlation between Academic Composite and Education reported earlier is 0.561. While these tests may differ in reliability, the correlations are quite similar.

I conclude that there is no evidence that the correlation between elementary and secondary school test scores and eventual educational attainment changed for men born between 1910 and 1945. It may have increased or decreased for men born after 1945. If schools are paying more attention to test scores, the correlation could be increasing. Or if instructional methods geared to individual differences had the expected effect, the correlation could be decreasing. No data seem to exist on whether the correlation of the test performance with educational plans is changing. The absolute effect of elementary school test scores is probably declining because the variance of educational attainment is declining.

Grade to Grade Changes

In theory, the correlation between test scores and eventual education should be higher for tests given close to school completion than for tests given earlier, since if cognitive abilities affect decisions to stay in school or drop-out, it should be current ability rather than past ability that counts. To test this I looked at the Talent sample analyzed by Jencks and Brown (forthcoming). It includes 453 9th graders, 445 10th graders, 417 11th graders, and 492 12th graders with Academic Composite scores and educational attainment five years after expected high school graduation. Scores on the Academic Composite correlated 0.567 with Education among 9th graders, 0.517 among 10th graders, 0.520 among 11th graders, and 0.527 among 12th graders. These differences are insignificant.

In fact, there is little evidence that tests given after 6th grade predict better than those given in 6th grade. Sixth grade Terman or Otis scores correlate 0.541 with Education for Olneck's 150 Kalamazoo men born 1934-38. Eleventh grade Academic Composite correlates 0.561 with Education for 28 year old Talent men born around 1944. Eleventh grade Henmon-Nelson scores correlate 0.446 with Education for 24 year old Wisconsin men born around 1940. The Wisconsin value is somewhat lower than the others, but no lower than many of the correlations in Table 1. Of course the samples tested in higher grades exclude men who did not reach these grades, lowering the correlation. But Jencks and Brown (1975) also report that when Talent retested a sample of 9th graders in 12th grade, the 12th grade scores predicted Education no better than the 9th grade scores.

If academic ability develops differently for individuals, I would expect twelfth grade test scores to be more highly correlated with Education than earlier test scores, since they are nearer in time to college admissions. The fact that twelfth grade tests do not predict education any better than sixth grade tests suggests that while abilities may develop at different rates in different individuals, the stable abilities measured by these tests are the ones that affect educational attainment. Apparently the tests used in these surveys measure such stable abilities as well in the sixth grade as they do in the twelfth grade.

Overall Effects of Adolescent Ability

I showed earlier that there are no consistent non-linear relationships between early tests and Education. However, as test scores increase, the standard deviation of Education increases in both the Talent and the Kalamazoo surveys. In Talent, this could result from not having data on those who got less than eleven years of education. This is less likely to be an issue in the Kalamazoo survey. The Kalamazoo results suggest that individuals with low test scores rarely attain the highest levels of education. Individuals with high test scores more often end up with very little education.

Table 4.7 presents regressions of education on test scores in eight surveys. The first line in each panel shows the bivariate regression of education on test scores. Comparisons of Talent, Kalamazoo, Wisconsin, and EEO imply that students whose elementary or secondary school scores differ by one point will differ by anywhere between 0.05 and 0.13 years of education. These surveys differ greatly in their representativeness. The less representative twins and siblings surveys show stronger relationships between test scores and schooling. The weakest relationship, shown by the Wisconsin survey, probably results in part from the fact that Sewell and Hauser excluded both men who were still in school at 24 and men who did not reach 12th grade from their sample. If one allows for the fact that all these samples are restricted to men with some minimal level of schooling, it seems safe to conclude that a one point difference in test score in the population as a whole implies a difference of 0.10 years in educational attainment, and perhaps even more. A one standard deviation difference in test score thus implies a difference of at least 1.5 years of schooling.

Table 4.7

Regressions of Education on Test Scores in Different Samples

Sample (Year of Test Score)		Coefficient for Test Score		% Reduction in Test Score Coefficient	Other Variables Controlled											R^2
		B	Beta		WHITE	POPED	POPOC	MOMED	PARINC	HEDAB	SIBS	POPED* SIBS	POPOC*TEST	TEST^2	ALL BKG[13]	
Talent Representative (1960)	1)	.09357[3]	.561	--												.314
N=839	2)	.08093	.485	13.5		X	X	X	X							.366
	3)	.08071	.484	13.7	X	X	X			X	X	X		X		.392
Wisconsin (1957)[4]	4)	.0526[3]	.446	--												.199
N=1789	5)	.0433	.367	17.7		X	X	X	X							.277
EEO (1955)[5]	6)	.08501[2]	.480	--												.230
N=538	7)	.06704	.378	21.1		X	X	X	X[i]							.309
Mälmo (1938)[9]	8)		.403[11]	--												.162
N=707	9)		.257[12]	36.2		X	X[10]				X		X			.410
Inequality (1940)	10)[6]		.550	--												.303
	11)		.440	20.0		X	X									.392
	12)[8]		.415	24.5											X	,630
	12A)[8]		.227	58.7											X	,564
Talent Brothers (1960)	13)	.11492	.632	--												.399
N=198	14)	.09600	.528	16.5		X	X				X					.468
	15)	.07843	.431	31.8											X	
Kalamazoo Brothers (1928-52)	16)	.103[3]	.576	--												.332
N=692	17)	.081	.456	21.4		X	X				X					.443
	18)	.059	.331	42.7											X	
Talent MZ Twins (1960)	19)	.128	.621	--												385
N=332	20)	.053	.251	58.6											X	

Coefficients less than twice their standard errors are in brackets.

Variables are: WHITE = White; POPED = Father's Education; POPOC = Father's Occupation; MOMED = Mother's Education; PARINC = Parent's Income; HEDAB = Male Head Absent; SIBS = Siblings; ALL BKG = All Background Controlled By Sibling Differences Regressions.

Notes for Table 4.7

1. A 13 item factor-weighted "acquisition" index of possessions in the respondent's high school household.
2. A 20 item test administered by ETS. Stice and Ekstrom (1964) estimate its reliability to be 0.82. I converted the scores to SD=15 for these computations.
3. Test was standardized to approximate mean of 100 and S.D. of 15.
4. Wisconsin results from Table 4-3, Sewell and Hauser (1975).
5. Karl Alexander kindly supplied me with their correlations, means, and standard deviations (Alexander, Eckland and Griffin, 1975), from which I computed the full equations in their model.
6. Table B-1, *Inequality*. Not corrected for attenuation.
7. Figure B-1, *Inequality*. Not corrected for attenuation.
8. Figure B-7, *Inequality*. Correlations corrected for measurement error. Upper and lower estimates.
9. Mälmo study. All variables standardized to $\bar{x} = 0$, S.D. = 1.
10. Four category scale based on father's occupation, 1937 family income, number of children at home, and appearances on the social welfare register of the Malmo schools.
11. Education had only four categories: $ED < 8$, $8 \leq ED \leq 10$, $11 \leq ED \leq 14$, $ED \geq 15$. Data available for 72% of population. Education correlated 0.814 with level of education defined as *type* of schooling completed. Test score was based on a group intelligence test (Hallgran, 1939).
12. Fagerlind's variables were all defined to have a mean of zero. Since a Father's Occupation X Test Score term was in their equation, with a standardized coefficient of 0.174, 0.257 is the Test Score coefficient when Father's Occupation is at the mean.
13. This column controls all background by regressing ΔU on ΔQ, where ΔQ is the difference between two brothers test scores, and ΔU is the difference between their educational attainments. Coefficients are standardized using S_Q and S_U not $S_{\Delta Q}$ or $S_{\Delta U}$.

Test scores are correlated with educational success partly because both are affected by common causes. I turn now to estimating the impact of tests on schooling after controlling for the influence of family background and genotype on both test scores and schooling.

Family Background. Family background is comprised of all the genetic and nongenetic characteristics that make siblings alike. Some of these characteristics are measured in the surveys in Table 4.7, but the characteristics which are measured differ from survey to survey. Table 4.7 shows that controlling measured family characteristics reduces the coefficient of test scores between 13 and 36 percent in the eight surveys. Eliminating the extreme Mälmo value, which is based on Swedish data, the reduction ranges from 13 to 21 percent and averages 17 percent. Thus, about 83 percent of the apparent effect of test scores is independent of the background characteristics measured in Table 4.7.

None of the surveys in Table 4.7 measures all of an individual's family characteristics. If the measures in these surveys captured the full effect of family background, then regressions of Education on these measured characteristics would yield R^2s as large as the intraclass correlations between siblings' educational attainments, assuming brothers do not affect one another. The R^2s are always much smaller than the intraclass correlations in our samples of brothers.8/

We may fully control family background by analyzing the relationship between test scores and education within pairs of brothers. Regressions of brother differences in Education on brother differences in Test Scores yield test score coefficients free from bias by factors that are uniform for the whole family. Table 4.7 shows that controlling family background in this way reduces the effects of test scores 32 percent below the zero order relationship in the Talent brothers survey.

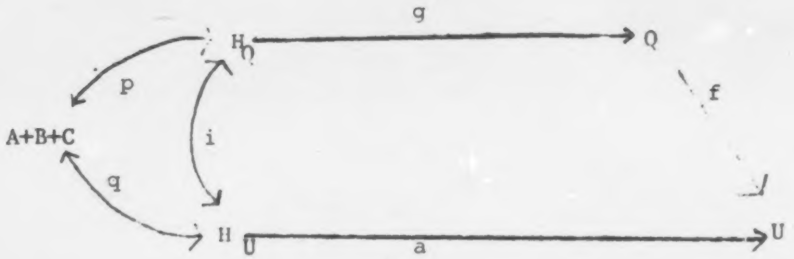
8/ See also Hauser and Featherman (1976: 117)

The reduction is 43 percent in the Kalamazoo brothers survey. This means that about 57-68 percent of the observed correlation between test scores and education is independent of family background. This estimate narrows the range that Jencks et al. considered in Inequality,which ran from 41 to 75 percent.

The reduction in the test score coefficient when all family background is controlled suggests that the unmeasured family characteristics affecting test scores and education are positively correlated with one another. This is illustrated
adapted from Chapter 2.
in Figure 1,/ Test scores in eleventh grade are represented as Q and Education as U. H_Q is the weighted sum of all family characteristics that brothers share and that affect test scores. H_U is the weighted sum of the family characteristics shared by brothers that affect Education after test scores are controlled. The correlation between H_Q and H_U is 0.513 in the Talent brothers sample. Olneck has estimated this correlation to be 0.618 for the Kalamazoo brothers. Family characteristics that increase brothers test scores are thus correlated with those that increase brothers education net of their test scores.

What are the family characteristics measured by H_Q and H_U? One possibility is that certain families socialize their children to attend school longer even if their children do not differ from other families'children in ability. We might expect this in families where the parents had more education than average. If this were the case, the unmeasured characteristics that affect educational attainment and test scores should be positively correlated with father's education or mother's education. Figure 1 illustrates the relationship of Father's Education (A), Father's Occupation (B), and Number of Siblings (C) to H_Q and H_U . These measured family characteristics explain an average of 30 percent of the variance in H_Q and 47 percent of the variance in H_U in the Talent and Kalamazoo brothers samples. Thus these measured characteristics only partially explain the

Figure 1: Path model of the sources of resemblance between brothers' education.



	a	f	g	i	p	q
Kalamazoo Brothers	.579	.331	.685	.618	.532	.664
Talent Brothers	.515	.431	.762	.513	.558	.702

A= Father's Education
B = Father's Occupation
C = Siblings
Q = Test Score
U = Education
H_q = family traits that influence test scores
H_u = family traits that influence education with test scores controlled
P = multiple correlation of H_q with father's education, father's occupation, siblings
q = multiple correlation of H_U with father's education, father's occupation, siblings

effects families have on their children's ability and education.

Another possibility is that the unmeasured characteristics are genetic, since siblings have half their genes in common. Jencks and Brown (forthcoming) estimate that genotype has substantial effects on tests and on education. However, the genes that affect test scores do not appear to be the same ones that affect education, after test scores are controlled.

Genotype. Test scores and education are polygenically determined. Unfortunately, there is no way to identify directly most of the genes that affect test scores. This means we cannot examine individuals who are alike on specific relevant genes when estimating the effects of test scores on education. Rather, we must study individuals who have all their genes in common. Identical twins are the only humans with identical genes. However, identical twins usually grow up in the same family. Even if they are separated at birth, their families may be quite similar. Part of their environmental similarity results from their genetic similarity. Separating the genetic and non-genetic components of the test score-education relationship is therefore difficult.9/

In theory, regressing the educational differences between a pair of MZ twins on the test difference between the same pair of twins provides a coefficient that cannot be biased by either genes or other common background characteristics. The Talent MZ twins sample in Table 4.7 is the only one with both education and an early test score for MZ twins. Controlling all genes and background reduces the coefficient for test scores from 0.128 to 0.053, a 59 percent reduction. This is a

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9/ See Jencks and Brown (forthcoming) for a discussion of these issues.

substantial reduction. It is considerably higher than the 32 percent reduction for the Talent 28 year old brothers where one-half the genes and background are controlled. It is also higher than the 43 percent reduction for the Kalamazoo brothers, also controlling one-half the genes and background. Indeed, it is the same as Jencks et al.'s upper estimate of 59 percent in Inequality. Comparisons of the Talent MZ Twins sample with the sibling sample in Table 4.7 therefore imply that genes make a small but not inconsequential contribution to the relationship of Test Scores to Education.

The results just presented do not tell us very much about the overall effect of genotype on either test performance or education. The results could arise because genotype had small effects on both test scores and education. Alternatively, genes could have substantial effects on both test scores and education, but the two sets of genes could be quite different. Genes may differentially affect educational attainment even among individuals with the same cognitive skills. The question of interest is how strongly the genes that affect education are correlated with the genes that affect test scores. Jencks and Brown have estimated this correlation to be 0.768. Their data imply that this correlation is almost entirely due to the fact that test scores affect education. The genes that affect education directly (i.e. with test scores controlled) are not the same as those that affect test scores.

Jencks and Brown's data imply that only a small fraction of the relationship between tests and educational attainment arises because the same genes affect test scores and education. If this is true, parents who provide their offspring with a genotypic advantage for test scores may not provide their offspring with a genotypic advantage for education. But given the small sample and large number of assumptions required to derive these results, they should be treated with special caution.

It is not possible to identify how genotype affects test scores or how genotype affects educational attainment. The Talent tests were given during high school. Genotype could easily have its effects on tests and education by affecting home and school environments that develop ability and educational success.

Explanations of Test Score Effects

The results just presented show that more than half of the relationship of academic ability to schooling is independent of the socio-economic character of an individual's family and any other characteristics common to brothers in the same family. I turn now to explaining how ability exercises its influence on an individual's schooling. Table 4.8 presents the basic data. The top panels present comparable regressions from three large surveys.

Grades. The skills measured by test scores influence educational attainment partly because they affect a student's success in high school. Controlling for high school Grades reduces the coefficient for test scores 14 percent below its level with only background controlled in the Talent survey. The reduction is 38 percent in the EEO and 55 percent in Wisconsin. Grades are not standardized over schools. The greater importance of grades in the Wisconsin results could be due partly to the fact that Wisconsin grades are school reports, while Talent and EEO have self-reports. The zero-order correlation of Grades with Education is higher in Wisconsin than Talent and EEO. Grades were also measured by class standing in Wisconsin and EEO, which could partly explain their greater importance in both these surveys than in Talent. Grades are cumulative through sophomore year in EEO, the middle of junior year in Talent, and senior year in Wisconsin.

Others influence. Individuals with high test scores report that they receive more teacher encouragement, parent encouragement, and friends encouragement to stay in school than low scoring individuals report. This may be partly due to high scoring individuals seeking out persons who will provide encouragement, but it may also be due partly to high scoring individuals receiving more encouragement

Table 4.8

Regressions of Education on Test Scores in Different Samples

Sample (Year of Test Score Data)		Coefficient for Test Score B	Coefficient for Test Score Beta	% Reduction in Test Score Coefficient	PERSONALITY	HSCURICULUM	GRADES	T-INFLUENCE	P-INFLUENCE	F-INFLUENCE	EDPLANS	OCPLANS	EARNPLANS	POFOC*GRADES	MEASURED BKG	R²
Talent Representative (1960) N=839	1)	.08093	.485	--											X[1]	.366
	2)	.06989	.419	13.6			X								X	.391
	3)	.05808	.348	28.2			X	X	X	X					X	.431
	4)	.04963	.297	38.7			X	X	X	X	X	X			X	.469
Wisconsin (1957) N=1789	5)	.0433	.367	--											X[3]	.277
	6)	.0194	.164	55.2			X[2]								X	.371
	7)	.0116	.098	73.2			X	X	X	X					X	.465
	8)	.0066	.056	84.8			X	X	X	X	X	X			X	.541
EEO (1955) N=538	9)	.06704[4]	.378	--											X[6]	.309
	10)	.04129	.233	38.4			X[5]									.396
	11)	.03932	.222	41.4			X	X	X	X						.432
	12)	.03735	.211	44.3			X	X	X	X	X	X			X	.454
Talent Representative (1960) N=839	13)	.08071	.484	--											X[7]	.392
	14)	.06579	.394	18.5		X									X	.414
	15)	.05806	.348	28.1		X	X								X	.427
	16)	.05213	.312	35.4		X	X	X	X	X					X	.455
	17)	.04911	.294	39.2		X	X	X	X	X	X	X	X		X	.483
	18)	.04840	.290	40.0		X	X	X	X	X	X	X	X	X	X	.487
	19)	.04987	.299	38.2	X	X	X	X	X	X	X	X	X	X	X	.496
	20)	.07859	.471	2.6	X										X	.415

All coefficients are more than twice their standard errors.

Variable definitions: PERSONALITY = self ratings of Sociability (R-601), Social Sensitivity (R-602), Impulsiveness (R-603), Vigor (R-604), Calmness (R-605), Tidiness (R-606), Culture (R-607), Leadership (R-608), Self-Confidence (R-609), and Mature Personality (R-610); HSCURICULUM = High School Curriculum; GRADES = Grades; T-INFLUENCE = Teacher's Influence on Education; P-INFLUENCE = Parent's Influence on Education; F-PLANS = Friend's Educational Plans; EDPLANS = Educational Plans; OCPLANS = Occupational Plans; EARNPLANS = Earning Plans; MEASURED BKG = Measured Background

* See Flanagan, et al. (1962).

Notes for Table 4.8

1. Line 2, Table 4.7.
2. Rank in high school class obtained from high school records, expressed as a percentile, and transformed to produce an approximately normal distribution.
3. Line 5, Table 4.7.
4. I converted the EEO test to a SD = 15 for these coefficients.
5. Self-reported sophomore class standing in terms of quartile rank.
6. Line 7, Table 4.7.
7. Line 3, Table 4.7.

than low scoring individuals from the same people or at least perceiving that they receive more such encouragement. High scorers report more encouragement than low scorers even when they report the same grades. Controlling parents', teachers', and friends' reported influence explains an additional 15 percent of the effects of test scores in the Talent survey, 18 percent in the Wisconsin survey, but only 3 percent in the EEO survey. As a result, 72 percent of the effect of test score in Talent, 59 percent in the EEO, and 27 percent in Wisconsin is not explainable by differences in grades and the influence of others.

Own plans. Individuals with high test scores want to enter higher status occupations and plan to get more education than those with low scores. This is true even for individuals who report similar grades / and encouragement from others. Educational and occupational plans are, in turn, important determinants of actual educational attainment. After controlling for prior variables the standardized coefficient for educational plans is 0.160 for EEO sophomores, 0.230 for Talent juniors, and 0.331 for Wisconsin seniors. These coefficients suggest that plans for higher education become better predictors of actual attainment as men approach the transition from high school to college. This correlation **inevitably approaches** 1.00 as the time of decision approaches. Occupational hopes are somewhat less important. Controlling for prior variables, the standardized coefficients are 0.072 in the EEO survey, 0.069 in Talent, and 0.066 in Wisconsin. The Talent and Wisconsin coefficients are significant but the/EEO coefficient is not. Controlling educational and occupational plans explains an additional 10 percent of the test score effect in Talent, 12 percent in Wisconsin, but only 3 percent in the EEO sample.

Grades, influence of others, and an individual's own plans together explain 39 percent of the effects of test scores in the Talent survey and 44 percent of the test score effect in the EEO survey. These values agree quite well, but are considerably less than the 85 percent explained in the Wisconsin survey. The difference is explainable largely by the higher correlations between the intervening variables and Education in the Wisconsin survey than in Talent and EEO. The higher Wisconsin correlations could be due to the fact that the Wisconsin survey measured grades, plans and the influences of others in 12th grade, whereas Talent measured them in 11th grade and EEO measured them in 10th grade.

The Wisconsin and EEO data that I had did not include information about a student's high school curriculum.[10] Nor did they include measures of his personality. Both of these are available in the Talent survey and further explain how test scores affect educational attainment. The bottom panel in Table 4.8 presents regressions designed to assess their importance.

High school curriculum. Higher scoring individuals are more likely to be in a college curriculum. I have assumed that test scores influence curriculum placement, which in turn affects grades, encouragement from others, and the individual' own plans. These, in turn, affect his educational attainment. About 19 percent of the effect of test scores depends on the fact that scores affect curriculum placement.[11] Lines 14 through 18 show that even after curriculum is controlled, grades, encouragement from others, and an individual's own plans continue to explain the effects of test scores on education, but somewhat less so than when curriculum is excluded.

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[10] Both surveys have a measure of curriculum, but not in available samples comparable to Talent. See Alexander and Eckland (1974) and Hauser, Sewell and Alwin (1976).

[11] Rosenbaum (1976) argues that curriculum placement also affects IQ. If this is true, controlling curriculum as I have done estimates the maximum that ability affects education by influencing curriculum placement. Rosenbaum's findings rely on a single school which could have atypically large curriculum effects. With 98 schools and better controls for background and initial ability, Jencks and Brown (1975) find that curriculum placement does not affect their six 12th grade Talent tests.

Personality characteristics. Test scores may be correlated with noncognitive traits or may affect the development of noncognitive traits that boost an individual's educational prospects. To test this I examined Talent's self-ratings of 10 personality characteristics; Sociability, Social-Sensitivity, Impusliveness, Vigor, Calmness, Tidiness, Culture, Leadership, Self-Confidence, and Mature Personality. An individual's score on each characteristic was inferred from agreement with statements containing behavioral adjectives to exemplify the characteristic. Mueser describes these measures in more detail in Chapter 5. Leadership and Mature Personality have the largest coefficients when all of these variables are entered after background is controlled. In general, however, these characteristics explain little of the effect of Test Scores on Education. Controlling them explains less than 3 percent of the test score effect (compare lines 13 and 20).[12] Controlling additional characteristics could explain much more of the test score effect. But none of my analyses suggest that test scores are simply proxies for personality characteristics. Nor do tests affect education by affecting the traits I measured. This suggests that the use of tests by schools does not "cool out" individuals on the basis of these Personality traits. Other personality characteristics may, of course, still be important.

Interactions. Interactions among the determinants of education do not appear to be very important. I investigated 21 possible two-way multiplicative interactions in Talent between the 5 background variables, test scores, and grades. Only two, Father's Education X Siblings and Father's Occupation X Grades, were significant, and neither of these included Test Scores. I also examined regressions in subsamples having white collar and blue collar fathers in the Talent and Kalamazoo surveys. The test score coefficient never differed significantly for white collar and blue collar fathers. Fagerlind also checked for interactions in his Swedish sample and only the Father's Occupation X Test Score interaction was significant. Testing for a large number of interactions will often result in some having coefficients more than twice their standard error by chance alone. Thus, without replication, I place little importance on any of these interactions.

The findings in this section can be summarized as follows: First, despite the allegedly increasing importance of tests in American society, the correlation between test scores and educational attainment appears to have remained stable from shortly after the turn of the century at least through the early 1950s. Tests given as early as the sixth grade also appear to predict educational attainment as well as tests given later in high school.

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[12] This comparison controls only that part of personality that is independent of measured background. Entering the 10 personality traits after test scores, when no background controls are entered, reduces the standardized test score coefficient from 0.561 to 0.543.

Second, high scoring individuals have greater educational opportunity than low scorers. A 15 point test score advantage in elementary or secondary school is associated with an 0.8 to 1.9 year educational advantage in adulthood. Academic ability is a more important determinant of educational attainment than measured socio-economic background in the United States. Yet inequalities in test scores do not contribute as greatly to inequalities in educational attainment as some may think. Table 4.7 shows that not controlling for any other variables, test scores explain only 20 - 33 percent of the variance in educational attainment in our best US samples. This means that the standard deviation of educational attainment among those who have the same test scores averages only 11-18 percent less than in the population as a whole.

Third, somewhere between 13 and 36 percent of the bivariate relationship between Test Scores and Education results from high scoring individuals coming from families that are economically more successful, more educated, smaller and more stable. Another 15 to 20 percent can probably be explained by additional characteristics of the family that are unmeasured in national surveys. These unmeasured characteristics may well involve ways of socializing children. They may also involve genetic differences among families.

Fourth, after controlling for family background, more than half the correlation between academic ability and education remains. Individuals who do not differ in family background, but who differ by one standard deviation in test scores, typically get 0.8 to 1.2 additional years of education. Our estimates show that about 20 percent of this effect results from high scoring individuals having favorable high school curriculum placement. Between 13 and 55 percent can be explained by the higher grades they make. Another 15 to 20 percent can be explained by the apparent encouragement they get from others. Another 2-10 percent may be explained by their own plans.[13] Personality self-assessments and interactions among the determinants of educational attainment seem to be of little importance.

Ability and Occupational Status

We have seen that a man's adolescent academic ability substantially affects his chances for educational attainment. This section will show that academic ability affects a man's occupational chances primarily because it affects his educational attainment. After controlling for education, the effects of test scores on occupational attainment are quite small.

Tests in Different Grades

Test scores in sixth and eleventh grades appear to predict an individual's occupational status at age 28-39 equally well. Olneck reports that sixth grade Terman or Otis IQ's correlate 0.491 with the current occupation of 35-39 year old Kalamazoo men. Eleventh grade Academic Composite correlates 0.474 with the occupations of 28-year-old Talent men. Eleventh grade Henmon/Nelson scores correlate 0.376 with 28-year-old Wisconsin men's occupation. The comparability of these tests is unknown. The samples are also different. Nonetheless, the correlations do not suggest that tests given in late adolescence predict occupational status any better than tests given in early adolescence. This is consistent with the results when predicting Education.

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[13] The contributions estimated for grades, encouragement, and own plans come from regressions that exclude curriculum, since this is only available in Talent. The Talent data suggest that including curriculum would not appreciably alter the picture.

Status Throughout an Individual's Lifetime

Tests given early in adolescence seem to predict a man's occupational attainment equally well throughout his lifetime. Fägerlind reports that group IQ tests at age 10 correlate 0.277, 0.350, 0.389, 0.386, and 0.352 with occupation status at 25, 30, 35, 40, and 43 for his sample of 707 Swedish men. These correlations do not differ significantly. This is what I would expect if differences in ability have occupational returns primarily because they affect differences in length of schooling. Olneck reports that Terman or Otis IQ scores correlate 0.475 with first occupation and 0.453 with occupation at age 35-59 in his Kalamazoo sample with complete data.

Historical Changes in Test Score-Occupation Correlations.

Olneck correlated sixth grade Terman or Otis IQs with first occupation for four cohorts of Kalamazoo men who were born between 1919-1923 (N=219), 1923 -1928(N=288), 1929-1933 (N=199), and 1934-1938 (N=150). The correlations were 0.455, 0.371, 0.523 and 0.429. The unstandardized coefficients were 0.7060, 0.5041, 0.7749, and 0.8252. These coefficients do not differ significantly or in substantively interesting ways. He also correlated these mens' IQs with their current occupational status in 1973. The correlations were 0.342, 0.402, 0.488 and 0.491. The unstandardized coefficients were 0.5450, 0.5461, 0.6780, and 0.8366. Both the correlations and unstandardized coefficients are thus higher for men born more recently, though none of the coefficients differs significantly from the others. This suggests that test scores may be growing more important for occupational selection in maturity, but the evidence for this is very weak.

Overall Effects of Adolescent Ability on Occupation

The first regression in each panel of Table 4.9 shows that school tests explain between 12 and 23 percent of the variance in adult occupational status with no other variables controlled. The explained variance is higher in the Talent and Kalamazoo surveys than in Wisconsin and EEO. A one standard deviation test score difference is associated with a difference of a third to half a standard deviation in occupational status. The problem of estimating the overall effects of Test Scores on Occupation is to control that part of the Test Score-Occupation relationship which results from Test Scores and Occupation sharing common causes.

Part of the occupational benefit of having high adolescent test scores arises because both depend on family advantages. The surveys in Table 4.9 measure family characteristics somewhat differently. The parallel analyses of these surveys show that controlling most combinations of measured background reduces the test score coefficient between 12 and 25 percent. These measured family advantages do not fully reflect all family characteristics that jointly determine test scores an occupation. Regressions using sibling differences reduce the Test Score coefficient 39 percent below the bivariate level in the Talent Brothers survey, and 36 percent below in the Kalamazoo Brothers survey. These values fall in the 22-48 percent range estimated in Inequality. I conclude that almost two-thirds of the test score effect arises from causes independent of all family characteristics.

Table 4.9

Regressions of Occupation on Test Scores in Different Samples

Sample (Year of Test Score Data)		Coefficients for Test Score: B	Coefficients for Test Score: Beta	% Reduction in Test Score Coefficient	WHITE	POPED	POPOC	MOMED	PARINC	HEDAB	SIBS	ALL BKG	R^2
Talent Representative (1960)	1)	.77149	.474	--									.225
N = 839	2)	.67253	.414	12.8		X	X				X		.248
	3)	.67968	.418	11.9		X	X	X	X				.252
	4)	.67652	.416	12.3	X	X	X			X	X		.250
Wisconsin (1957)	5)	.6054	.376	--									.141
N = 1789	6)	.499	.310	17.6		X	X	X	X				.199
EEO (1955)	7)	$.54981^4$	.350	--									.123
N = 538	8)	.41405	.264	24.7		X	X	X	X				.189
Inequality (1940)	9) 1		.380	--									.212
	10) 2		.288	24.2		X	X						
	11) 3		.297	21.8								X	
	11A) 3		.198	47.9								X	
Talent Brothers (1960)	12)	.91282	.484	--									.234
N = 198	13)	.74810	.396	18.1		X	X				X		.270
	14)	.55916		38.7								X	
Kalamazoo Brothers (1928-52)	15)	.68509	.453	--									.206
N = 692	16)	.601	.397	12.3		X	X				X		.217
	17)	.436		36.4								X	

All coefficients are larger than twice their standard error.

Notes for Table 4.9

1. Derived from Figure B-1, Inequality. Not corrected for measurement error. Note that this is not the value shown in **Table** B-1 of Inequality, which was not used in later computations.
2. Figure B-1, Inequality. Excludes direct effect of IQ-11 on OC. Correlations not corrected for measurement error.
3. **Upper and lower estimates calculated from paths in Figure B-7, Inequality, corrected for measurement error.**
4. I converted the EEO Test to S.D. = 15 for these computations.

Explanations of the Effects of Academic Ability

Regressions from different surveys designed to test hypotheses about how adolescent test scores affect adult occupation are presented in Table 4.10.

Individuals' characteristics in high school. Differences in high school grades explain 12 to 51 percent of the effect of adolescent scores on eventual status. Grades are least important in the Talent representative survey, more important in the EEO survey, and still more important in the Wisconsin survey. As I mentioned earlier, these differences are probably partly traceable to differences in the time and method of measurement. Differences in encouragement by others that have nothing to do with grades explain an additional 3 to 15 percent of the occupation gap. Differences in the individual's own educational and occupational plans that are independent of grades and encouragement from others explain another 4 to 10 percent. The effects of encouragement from others and of one's own plans may be partly due to enrollment in a college curriculum, but the causal order is unclear. The personality self-assessments measured in the Talent survey explain less than 4 percent of the effect of test scores. This is probably a low estimate, since I examined so few measures of personality.

Education. The most important reason why individuals with different scores end up in different occupations is that they get different amounts of schooling. Controlling education alone reduces the test score coefficient 62.6 percent in the Talent representative sample. Also controlling grades, others' encouragement, own plans, high school curriculum and personality further reduces it to only 72.8 percent. The Talent brothers sample

Table 4.10

Regressions of Occupation on Test Scores in Different Samples

Sample (Year of Test Score Data)		Coefficient for Test Score		% Reduction in Test Score Coefficient	Other Variables Controlled																					R^2
		B	Beta		PERSONALITY	WHITE*TEST	HSCURICULUM	GRADES	T- INFLUENCE	P- INFLUENCE	F-PLANS	EDPLANS	OCPLANS	EARNPLANS	ED	EDPASTHS	BA	GRADED	POPOC*ED	TEST*ED	EARLYOC	WORKEXP	WORKEXP^2	MEASURED BKG	ALL BKG	
Talent Representative (1960)	1)	.67968	.418	--																				(1)		.252
N = 839	2)	.59553	.366	12.4				X																(1)		.252
	3)	.51160	.315	24.7				X	X	X	X													(1)		.268
	4)	.46849	.288	31.1				X	X	X	X	X	X											(1)		.292
	5)	.22390	.138	67.1				X	X	X	X	X	X		X									(1)		.305
Wisconsin (1957)	6)	.499	.310	--																				(1)		.199
N = 1789	7)	.247	.153	50.5				X																(1)		.255
	8)	.172	.107	65.5				X	X	X	X													(1)		.300
	9)	.123	.076	75.4				X	X	X	X	X	X											(1)		.334
	10)	[.082]	[.051]	83.6				X	X	X	X	X	X		X									(1)		.426
EEO (1955)	11)	.41405	.264	--																				(1)		.180
N = 538	12)	.23002	.147	44.5				X																(1)		.246
	13)	.21563	.137	47.9				X	X	X	X													(1)		.285
	14)	.19741	.126	52.3				X	X	X	X	X	X											(1)		.303
	15)	[.04786]	[.031]	88.4				X	X	X	X	X	X		X									(1)		.415
Talent Representative (1960)	16)	.676[illegible]2	.416	--																				(2)		.250
N = 839	17)	.67778	.417	--		X																		(2)		.252
	18)	.51874	.319	23.3		X	X																	(2)		.279
	19)	.44884	.276	33.7		X	X	X																(2)		.297
	20)	.41924	.258	38.0		X	X	X	X	X	X													(2)		.314
	21)	.40467	.249	40.2		X	X	X	X	X	X	X	X	X										(2)		.323
	22)	.16570	.102	75.5		X	X	X	X	X	X	X	X	X	X		X	X						(2)		.458
	23)	.16664	.102	75.4		X	X	X	X	X	X	X	X	X	X		X	X	X					(2)		.462
	24)	.16660	.102	75.4		X	X	X	X	X	X	X	X	X	X		X	X	X			X		(2)		.462
	25)	.16791	.103	75.2		X	X	X	X	X	X	X	X	X	X		X	X	X			X	X	(2)		.465
	26)	.17726	.109	73.8	X	X	X	X	X	X	X	X	X	X	X		X	X	X			X	X	(2)		.473
	27)	.65006	.400	3.9	X																			(2)		.277
Talent Representative (1960)	28)	.67968	.418	--																				(1)		.252
N = 839	29)	.25441	.156	62.6											X		X	X						(1)		.437

Table 4.10 Continued

Regressions of Occupation on Test Scores in Different Samples

Sample (Year of Test Score Data)		Coefficient for Test Score B	Coefficient for Test Score Beta	% Reduction in Test Score Coefficient	PERSONALITY	WHITE*TEST	HSCURICULUM	GRADES	T-INFLUENCE	P-INFLUENCE	F-PLANS	EDPLANS	OCPLANS	EARNPLANS	ED	EDPASTHS	BA	GRADED	POPOC*ED	TEST*ED	EARLYOC	WORKEXP	WORKEXP2	MEASURED BKG	ALL BKG	R^2
Talent 28 Year Old Brothers (1960) N = 198	30)	.74810	.396	--																				(3)		.270
	31)	.06670	.035	91.1											X									(3)		.519
	32)	.55916		--																					X	
	33)	.04890		91.3											X										X	
Kalamazoo Brothers (1928-52) N = 692	34)	.601	.397	--																				(3)		.217
	35)	.247	.163	58.9											X	X	X							(3)		.369
	36)	.228	.151	62.1											X	X	X				X			(3)		.410
	37)	.436		--																					X	
	38)	.226		48.2											X	X	X								X	
	39)	.220		49.5											X	X	X				X				X	

Coefficients less than twice their standard error are in brackets.

Variable identifications not seen in previous tables are: ED = Years of Education; EDPASTHS = Years of Education Past High School; BA = College Graduation; GRADED = Years of Graduate Education; EARLYOC = First Occupation; WORKEXP = Work Experience.

(1) Indicates controls for Father's Education, Father's Occupation, Mother's Education, Family Income.

(2) Indicates controls for Father's Education, Father's Occupation, Father Absent, Race, Siblings.

(3) Indicates controls for Father's Education, Father's Occupation, Siblings.

and the Kalamazoo brothers sample also show substantial reductions in the test score coefficient with only education controlled. These results suggest that test scores affect a man's occupational status primarily by influencing his educational attainment.

Early occupation. Olneck surveyed Kalamazoo men when they were older than Wisconsin, Talent, and EEO men. In addition to asking their current occupation, he also asked for their first occupation. Controlling first occupation reduces the test score coefficient only an additional 1 to 3 percent beyond the reduction due to education. Thus the occupational advantage of middle-aged men with high scores early in school does not appear to come from the initial advantage they receive by beginning their careers at a higher level, except insofar as their higher educational level starts them in higher status jobs.

In the Wisconsin and EEO surveys, the effect of test scores on adult status does not reach significance after controlling for the intervening variables. Even in Talent, if two men differ by 15 points on the composite test but have the same personality, high school curriculum, grades, encouragement from others, own plans, and education, their Duncan scores at 28 typically differ by only 2.5 points.

Interactions. The effects of test scores on occupational status appear to be essentially additive. I investigated the ten possible multiplicative interactions in Talent between the five background variables, the three possible interactions between Test Scores, Grades, and Education, and the fifteen possible interactions between the five background variables and Test Scores, Grades, and Education. One would expect one or two of 28 interactions to reach significance at 0.05 by chance alone. In fact, two of the 28 had coefficients more than twice their standard errors. Test Score had a smaller effect when respondents were white and Education had smaller effects for sons of high status fathers. I also examined subsamples of respondents having blue collar and white collar fathers. The test score coefficient did not differ significantly from one subsample to the other in either Talent or Kalamazoo. The most interesting finding with regard to interactions is the absence of any interaction between Education and Test Scores in either the Talent or Kalamazoo surveys. This suggests that at least within the range of cognitive skills found in these two surveys, the occupational payoff to education is no greater for brighter students than for slow learners who persist in school.

The findings in this section lead me to several conclusions:

First, American society has probably not allocated high status occupations more meritocratically since the 1900s, at least if early test scores are measures of merit. This conclusion must be tentative, however, given the weak evidence available.

Second, tests given early in an individual's schooling predict his adult occupational status as well as tests given during the last year of high school. Test scores predict occupational success in maturity as well as in early adulthood.

Third, a man's ability in high school has important effects on his later occupational status, but these effects are almost entirely explained by the amount of schooling he gets. The amount of schooling he gets can be traced largely to the effects tests have on curriculum placement, grades, encouragement from others, and his/ high school plans. Men who fail to convert their ability advantage into longer schooling will differ very little in their adult occupational status from men with similar schooling but lower initial scores. As we shall see, they may differ much more in their Earnings, even if they do not differ in occupational status.

Ability and Earnings

Historical Changes in the Test Score-Earnings Correlation

Despite the importance of tests in American society, there has never been a historical study of the relationship between adolescent test scores and adult earnings. Our data suggest that this relationship has been quite stable for cohorts of men born from shortly after the turn of the century through the late 1930s. The correlations between sixth grade Terman/ or Otis IQs and Ln Earnings in adulthood for cohorts of Kalamazoo men born in 1919-1923, 1923-1928, 1929-1933, and 1934-1938 are 0.324, 0.339, 0.455, and 0.337. These men were aged 35-54 at the time of the survey. The unstandardized coefficients show the same pattern. The coefficients do not differ statistically from one another. While these data are far from ideal, they suggest that for the period of time covered, the historical relationship has been fairly stable. The most surprising finding is that this topic has never been the subject of a major investigation.

Elementary and Secondary School Tests

Third, sixth, eleventh, and twelfth grade tests do not appear to differ in their prediction of young adults' earnings, although the data are not ideal. Test scores at age 10 correlate 0.220 with the Ln Earnings of Fagerlind's Swedish men at 29 and 0.343 with Ln Earning at 34. Sixth grade Terman/ or Otis IQs correlate 0.319 with the 1973 Earnings of 35-39 year old Kalamazoo men. Eleventh grade academic composite correlates 0.147 with the hourly earnings of 28 year old Talent men. Eleventh grade Henmon/Nelson scores correlate 0.163 with the earnings of Sewell and Hauser's 28 year old Wisconsin men.

Earnings Later in an Individual's Life

The correlation between test scores and earnings appears to keep rising up to the age of 30 or 35 and to stabilize after that. Sewell and Hauser report that Wisconsin men's 11th grade Henmon/Nelson scores correlated 0.096, 0.125, and 0.166 with their earnings at about 26, 27, and 28. These correlations continue to rise up to the age of 32, though exact values are not available (Hauser and Daymont, **in process**).

Fagerlind followed up a single cohort of Swedish men and reported correlation between his group IQ test at age 10 and income at ages 24, 29, 34, 39, and 42 of 0.082, 0.222, 0.343, 0.333, and 0.396. Olneck found that sixth grade Terman/Otis scores correlated 0.319, 0.476, 0.338, and 0.283 with the 1973 earnings of Kalamazoo men aged 35-39, 40-44, 45-50, and 50-54. The decrease for older Kalamazoo men is not statistically significant. This same pattern is found for the unstandardized coefficients and also for Ln Earnings.

I conclude that the test score-earnings correlation is probably fairly stable for men between 35 and 55. It could fall after 55. This conclusion must, however, be very tentative because of the limited evidence currently available.

Overall Effects of Academic Ability

Tables 4.11 and 4.12 present regressions of Earnings and Ln Earnings on Test Scores in different samples. In the sections that follow I will present some results for Earnings and some for Ln Earnings. Earnings allows me to make comparisons with a larger number of other surveys that include test scores, while Ln Earnings is more comparable with results in other chapters of this report.

Estimates of the bivariate association of Test Scores with Earnings or Ln Earnings differ considerably from sample to sample. The test score coefficient in the EEO survey does not reach statistical significance. A 15 point increase in test scores is associated with an increase of 3 percent of the mean in the EEO sample, 6 percent of the mean in Wisconsin, 8 percent in the Talent representative sample, 16 percent in the Talent and Kalamazoo brothers samples, and 18 percent in the Malmo sample.

Estimates of the effects of controlling measured background on the test score coefficient also vary from sample to sample. The coefficient is negative (but insignificant) in the EEO survey. The coefficient increases slightly in the Talent brothers sample. This could be due to sampling error since there are only 198

Table 4.11

Regressions of Earnings on Test Scores in Different Sample

Sample (Age for Earnings)		Coefficient for Test Score		% Reduction in Test Score Coefficient	Other Variables Controlled								R^2
		B	Beta		WHITE	POPED	POPOC	MOMED	PARINC	HEDAB	SIBS	ALL BKG	
Talent Representative (28)	1)	.02887	.203	--									.041
N = 839	2)	.02310	.162	20.0		X	X				X		.052
	3)	.02438	.171	15.6		X	X	X	X				.057
	4)	.02338	.164	19.0	X	X	X			X	X		.055
Wisconsin (28)	5)	29.2	.163	--									.026
N = 1789	6)	24.5	.137	16.1		X	X	X	X				.048
EEO (30)	7)	[21.4]	[.070]	--									.005
N = 538	8)	[-13.5]	[-.045]	--		X	X	X	X				.092
Inequality (25-64)	9)[1]		.239	--									.057
	10)[1]		.187	21.8		X	X						
	11)[2]		.194	18.8								X	
	11A)[2]		.232	2.9								X	
Talent (28)	12)	.04969	.365	--									.133
N = 198	13)	.05154	.379	-3.7		X	X				X		.150
	14)	.03570		28.2								X	
Kalamazoo Brothers (35-59)	15)	179	.359	--									.128
N = 692	16)	156	.313	12.9		X	X				X		.142
	17)	170		5.0								X	

Coefficients less than twice their standard errors are in brackets.

Notes for Table 4.11

1. Computed from Figure B-1, Inequality.
2. Computed from paths in Figure B-7, Inequality. Lines 11 and 11A assume extreme values of m and v.
3. Talent Earnings are hourly earnings. Use of hourly earnings will underestimate the effects of worker's traits on annual earnings to the extent that the traits affect weeks worked and weeks worked affects annual earnings.

Table 4.12

Regressions of Ln Earnings on Test Scores in Different Samples

Sample (Age for Earnings)		Coefficient for Test Score B	Coefficient for Test Score Beta	% Reduction in Test Score Coefficient	WHITE	POPED	POPOC	MOMED	PARINC	HEDAB	SIBS	ALL BKG	R^2
Talent Representative (28) N = 839	1)	.00552	.203	--									.041
	2)	.00455	.167	17.6		X	X				X		.052
	3)	.00477	.176	13.6		X	X	X	X				.057
	4)	.00455	.168	17.6	X	X	X			X	X		.054
Mälmo (43) N = 707	5)	$.01209^2$	.396	--									.157
	6)	.00907	$.297^1$	25.0		X	X				X		.252
Talent (28) N = 198	7)	.01066	.356	--									.127
	8)	.01152	.385	-8.1		X	X				X		.147
	9)	.00996		6.6								X	
Kalamazoo Brothers (35-59) N = 692	10)	.0106	.360	--									.130
	11)	.0094		11.3		X	X				X		
	12)	.0105		0.9								X	

All coefficients larger than twice their standard error.

1. Fagerlind, Table 9. Respondents were about 43 years old in 1971 when income was measured.

2. Test scores converted S.D. = 15.

individuals in the sample. The remaining samples show reductions in the test score coefficient between 11 and 25 percent.

The pattern is similar when one uses sibling differences to estimate the effect of unmeasured as well as measured background characteristics. The coefficients in the difference equations in Tables 4.11 and 4.12 imply that background accounts for 1 to 5 percent of the apparent effect of test performance on earnings in the Kalamazoo brothers sample, 7 to 28 percent in the Talent brothers sample, and 3 to 19 percent in Jencks et al's Inequality estimates. The reduction with sibling controls is slightly less than with measured background controlled in the Kalamazoo brothers sample, but not in the Talent brothers sample. I conclude that controlling family background explains no more than a quarter of the bivariate association of test scores and earnings in our samples. The absolute bias is consistently small.

The overall effect of test scores on earnings with all background controlled appears to be substantively important. Each 15 point test score increase is associated with a 15 percent increase in Talent brothers' hourly earnings and a 16 percent increase in Kalamazoo brothers' annual earnings. The effects may be even larger than this, since I showed earlier that any single test is likely to understate the overall effect of the cognitive skills measured by a battery of tests, and since the overall variance in earnings is restricted in both samples.

Explanations of the Effects of Adolescent Ability

Tables 4.13 and 4.14 present regressions from different surveys designed to explain how adolescent test scores affect adult Earnings and Ln Earnings. These tables allow me to decompose the overall effect of tests into discrete components which explain how adolescent test scores affect a man's adult earnings. I will disregard the EEO results because the sample is small, the standard errors large, and the results anomalous.

Individual's characteristics in high school. Differences in high school grades explain 9 percent of the effect of test scores on earnings in the Talent representative sample, and 38 percent in the Wisconsin sample. As before, this difference is probably traceable to differences in the measure of grades and also the time of measurement. Differences in the encouragement given by others explain another 24 percent of the effect in the Talent representative sample and another 5 percent in Wisconsin. Educational and occupational plans account for another 11 percent in both Talent and Wisconsin. Being in a college curriculum explains 23 percent, assuming that test scores affect curriculum placement, which in turn influences grades, encouragement from others, and one's own plans. Personality self-assessments are more important for earnings than for either occupation or education. Controlling only measured background, personality explains 24 percent of the test score effect on earnings

Table 4.13

Regressions of Hourly or Annual Earnings on Test Scores in Different Samples

Sample (Age and Accounting Period for Earnings)		Coefficient of Test Score B	Coefficient of Test Score Beta	% Reduction in Test Score Coefficient	PERSONALITY	HSCURIC ULUM	GRADES	T-INFLUENCE	P-INFLUENCE	F-PLANS	EDPLANS	OCPLANS	EARNPLANS	ED	EDPASTHS	BA	GRADED	EARLYOC	WORKEXP	OCCUPATION	MEASURED BKG	ALL BKG	R²
Talent Representative (Hourly Earnings at 28) N = 839	1)	.02438	.171	--																	(1)		.057
	2)	.02220	.156	8.9			X														(1)		.058
	3)	.01636	.115	32.9			X	X	X	X											(1)		.072
	4)	.01374	.097	43.6			X	X	X	X	X	X									(1)		.079
	5)	[.00877]	[.062]	64.0			X	X	X	X	X	X		X							(1)		.086
	6)	[.00579]	[.041]	76.3			X	X	X	X	X	X		X						X	(1)		.099
Wisconsin (Annual Earnings at 28) N = 1789	7)	24.5	.137	--																	(1)		.048
	8)	15.2	.085	38.0			X														(1)		.054
	9)	13.9	.077	43.3			X	X	X	X											(1)		.055
	10)	11.1	.062	54.7			X	X	X	X	X	X									(1)		.064
	11)	[10.0]	[.056]	59.2			X	X	X	X	X	X		X							(1)		.070
	12)	[9.0]	[.050]	63.3			X	X	X	X	X	X		X						X	(1)		.076
EEO (Annual Earnings at 30) N = 538	13)	[-13.5]	[-.045]	--																	(1)		.092
	14)	[-18.9]	[-.062]	-40.0			X														(1)		.093
	15)	[-14.5]	[-.064]	-7.4			X	X	X	X											(1)		.102
	16)	[-14.8]	[-.065]	-9.6			X	X	X	X	X	X									(1)		.102
	17)	[-28.8]	[-0.95]	-113.3			X	X	X	X	X	X		X							(1)		.113
	18)	-30.1	-.099	-123.0			X	X	X	X	X	X		X						X	(1)		.124
Talent Representative (Hourly Earnings at 28) N = 839	19)	.02338	.164	--																	(2)		.055
	20)	.01811	.127	22.5																	(2)		.059
	21)	.01648	.116	29.5		X	X														(2)		.060
	22)	.01330	.093	43.1		X	X	X	X	X											(2)		.072
	23)	[.01221]	[.086]	47.8		X	X	X	X	X	X	X	X								(2)		.079
	24)	[.00730]	[.051]	68.8		X	X	X	X	X	X	X	X	X		X	X				(2)		.090
	25)	[.00741]	[.052]	68.3		X	X	X	X	X	X	X	X	X		X	X		X		(2)		.103
	26)	[.00501]	[.035]	78.6		X	X	X	X	X	X	X	X	X		X	X		X	X	(2)		.116
	27)	[.00438]	[.031]	81.3	X	X	X	X	X	X	X	X	X	X		X	X		X	X	(2)		.142
	28)	.01787	.125	23.6	X																(2)		.095

Table 4.13 Continued

Regressions of Hourly or Annual Earnings on Test Scores in Different Samples

Sample (Age and Accounting Period for Earnings)		Coefficient of Test Score		% Reduction in Test Score Coefficient	Other Variables Controlled																		R²
		B	Beta		PERSONALITY	HSCURIC	GRADES	T-INFLUENCE	P-INFLUENCE	F-PLANS	EDPLANS	OCPLANS	EARNPLANS	ED	EDPASTHS	BA	GRADED	EARLYOC	WORKEXP	OCCUPATION	MEASURED BKG	ALL BKG	
Talent Representative (Age 28; Hourly Earnings) N = 839	29)	.02310	.162	--																	(3)		.052
	30)	[.01103]	[.077]	52.3										X		X	X				(3)		.076
	31)	[.00738]	[.052]	68.1										X		X	X			X	(3)		.091
Talent Brothers (Hourly Earnings at 28) N = 198	32)	.05154	.379	--																	(3)		.150
	33)	.02840	.209	44.9										X							(3)		.205
	34)	.02792	.205	45.8										X						X	(3)		.209
	35)	.03570		--																		X	
	36)	[.01836]		48.6										X								X	
	37)	[.01773]		50.3										X						X		X	
Kalamazoo Brothers (Annual Earnings at 35-59) N = 692	38)	156	.313	--																	(3)		.142
	39)	83	.167	46.8										X	X	X					(3)		.202
	40)	55	.110	64.7										X	X	X				X	(3)		.270
	41)	55	.110	64.7										X	X	X		X		X	(3)		.273
	42)	170		--																		X	
	43)	134		21.2										X	X	X						X	
	44)	112		34.1										X	X	X				X		X	
	45)	113		33.5										X	X	X		X		X		X	

Coefficients less than twice their standard error are in brackets.

(1) Indicates controls for Father's Education, Father's Occupation, Mother's Education, Parental Income.

(2) Indicates controls for Father's Education, Father's Occupation, Father Absent, Race, Siblings.

(3) Indicates controls for Father's Education, Father's Occupation, Siblings.

Table 4

Regressions of Ln Annual Earnings or Ln Hourly Earnings on Test Scores in Different Samples

Sample (Age for Earnings)		Coefficient of Test Score		% Reduction in Test Score Coefficient	Other Variables Controlled																		R²
		B	Beta		PERSONALITY	HSCURIC	GRADES	T-INFLUENCE	P-INFLUENCE	F-PLANS	EDPLANS	OCPLANS	EARNPLANS	ED	EDPASTHS	BA	GRADED	EARLYOC	WORKEXP	OCCUPATION	MEASURED BKG	ALL BKG	
Talent Representative (Hourly Earnings at 28) N = 839	1)	.00477	.176	--																	(1)		.057
	2)	.00454	.167	4.8			X														(1)		.057
	3)	.00393	.145	17.6			X	X	X	X											(1)		.065
	4)	.00327	.121	31.5			X	X	X	X	X	X									(1)		.076
	5)	.00247	.091	48.2			X	X	X	X	X	X		X							(1)		.081
	6)	[.00190]	[.070]	60.2			X	X	X	X	X	X		X						X	(1)		.094
Talent Representative (Hourly Earnings at 28) N = 839	7)	.00455	.168	--																	(2)		.054
	8)	.00365	.134	19.8		X															(2)		.057
	9)	.00348	.128	23.5		X	X														(2)		.057
	10)	.00314	.116	31.0		X	X	X	X	X											(2)		.064
	11)	.00283	.104	37.8		X	X	X	X	X	X	X	X								(2)		.075
	12)	[.00202]	[.074]	55.6		X	X	X	X	X	X	X	X	X		X	X				(2)		.087
	13)	[.00204]	[.075]	55.2		X	X	X	X	X	X	X	X	X		X	X		X		(2)		.104
	14)	[.00158]	[.058]	65.3		X	X	X	X	X	X	X	X	X		X	X		X	X	(2)		.118
	15)	[.00136]	[.050]	70.1	X	X	X	X	X	X	X	X	X	X		X	X		X	X	(2)		.135
	16)	.00379	.140	16.7	X																(2)		.079
Talent Representative (Hourly Earnings at 28) N = 839	17)	.00455	.167	--																	(3)		.052
	18)	.00261	.096	42.6										X		X	X				(3)		.074
	19)	[.00192]	[.071]	57.8										X		X	X			X	(3)		.089
Talent Brothers (Hourly Earnings at 28) N = 198	20)	.01152	.385	--																	(3)		.147
	21)	.00678	.226	41.2										X							(3)		.195
	22)	.00667	.222	42.1										X						X	(3)		.200
	23)	.00996		--																		X	
	24)	[.00667]		33.0										X								X	
	25)	[.00652]		34.5										X						X		X	

Coefficients less than twice their standard error are in brackets.

Table 4.14 Continued

Regressions of Ln Annual Earnings or Ln Hourly Earnings on Test Scores in Different Samples

Sample (Age for Earnings)		Coefficient of Test Score		% Reduction in Test Score Coefficient	Other Variables Controlled																		R²
		B	Beta		PERSONALITY	HSCURICULUM	GRADES	T-INFLUENCE	P-INFLUENCE	F-PLANS	EDPLANS	OCPLANS	EARNPLANS	ED	EDPASTHS	BA	GRADED	EARLYOC	WORKEXP	OCCUPATION	MEASURED BKG	ALL BKG	
Kalamazoo Brothers	26)	.0094	.320	--																	(3)		.137
(Annual Earnings at 35-59)	27)	.0056	.191	40.4										X	X	X					(3)		.202
N = 692	28)	.0038	.129	59.6										X	X	X				X	(3)		.283
	29)	.0038	.129	59.6										X	X	X		X		X	(3)		.287
	30)	.0105		--																		X	
	31)	.0086		18.1										X	X	X						X	
	32)	.0072		31.4										X	X	X				X		X	
	33)	.0072		31.4										X	X	X		X		X		X	

Coefficients less than twice their standard errors in brackets.

(1) Indicates controls for Father's Education, Father's Occupation, Mother's Education, and Family Income.

(2) Indicates controls for Father's Education, Father's Occupation, Father Absent, Race, Siblings.

(3) Indicates controls for Father's Education, Father's Occupation, Siblings.

and 17 percent of the test score effect on Ln Earnings. This interpretation assumes that test scores affect these personality traits, but not vice versa. If the personality characteristics affect test scores, then 24 percent of the test score effect on Earnings and 17 percent of the effect on Ln Earnings is spurious. Regardless of which interpretation one prefers, personality characteristics correlated with a man's test scores are relatively more important for his earnings than for either his education or occupation. In absolute terms, however, the effects are still small.

Education. The lower panels of Tables 4.13 and 4.14 show that controlling only Education often decreases the Test Score coefficient substantially. This suggests that test scores may affect earnings primarily because they influence education. This cannot be the whole story, however, since even when we compare brothers with the same education, a 15 point difference in test performance results in a 10 percent difference in Hourly Earnings in Talent and a 13 percent difference in Annual Earnings in Kalamazoo. These effects might be larger in samples with more variable earnings than Talent and Kalamazoo.

Occupation. Test scores continue to affect a man's earnings even with occupation controlled. Thus if brothers have the same education and the same occupational status but have test scores that differ by 15 points, they differ by 10-11 percent in earnings.

Interactions. Interactions again appear to be of little importance. I investigated the same 28 interactions in the Talent survey for Ln Earnings that I described earlier for Occupation. None was significant at the 0.05 level. Regressions for subsamples of Talent and Kalamazoo respondents who had blue collar and white collar fathers also showed no significant differences in the Test Score coefficients. The most interesting finding again is that there is no significant Test Score by Education interaction in the Talent or Kalamazoo samples. If brighter students learn more in school, and if school learning were the basis for higher earnings, I would expect a positive Test Score by Education interaction.

These results lead me to several conclusions:

First, the correlation between test scores and earnings appears to have been stable for men born between World War I and World War II in Kalamazoo. There is no good evidence of change after that or in other areas of the United States.

Second, tests given in late elementary school predict a man's adult earnings as well as tests given in high school. Adolescent tests predict earnings at 35 to 54 somewhat better than for younger men.

Third, adolescent test scores have substantively important effects on earnings. A one point increase in test performance results in a one percent increase in adult earnings after family background has been controlled.

Fourth, differences in an individual's characteristics in high school and differences in educational attainment help explain the higher earnings of men with high test scores. But even after education and occupation are controlled, test scores appear to have substantively important effects on adult earnings. This is true in both the Talent and Kalamazoo brothers samples. A 10 point increase in test scores is associated with a 6-8 percent increase in earnings with education controlled. This may be an underestimate if (a) single tests do not capture the full effects of tests, or (b) if the effects are larger in samples with more variable earnings.

3. Effects of Adult Ability

I turn now to the importance of adult cognitive ability for adult success. This section has three parts. The first compares the relative importance of adult tests with adolescent tests. The second investigates the effects of adult ability on occupational status. The third describes the effect of adult ability on earnings.

(a) Adult Versus Adolescent Ability

It is not obvious whether economic success should correlate better or worse with adult tests than with adolescent tests. Correlations between adolescent and adult ability are typically in the range of 0.8-0.9 (cf. Bloom, 1964). These correlations suggest that adult success should show much the same relationship to adult tests as to adolescent tests. However, measures of adult ability incorporate not only the effects of early ability but the effects of unequal schooling.

Thus I would expect adult success to correlate higher with adult tests than with earlier tests. But the tests used to measure school and postschool ability are seldom alike. Even when they have the same content, tests may measure different things for adolescents and adults. Adolescents who can be located as adults may also have environments which have changed less than those of adolescents who cannot be located, so their adult success may be more predictable from early tests than is the case for those who could not be located.

The Malmo study includes a measure of ability at age 10, a military test of ability ten years later (when respondents had finished almost all of their schooling) and measures of occupational status and earnings at 25, 30, 35, 40, and 43. The two tests correlated 0.745, which is lower than in most American studies of representative samples. Occupational status had an average correlation of 0.351 with scores at 10 and 0.486 with scores at 20. For income, the correlations were 0.274 vs. 0.333. The adult test thus had somewhat higher correlations with both occupation and income than the test at age 10.

Fägerlind (1975) reports that those who completed university had some of their formal education after "adult" ability was measured. If so, Fagerlind's data may understate the difference between adolescent and adult tests.

There are no American studies which have both adolescent and adult test scores and which follow up men's economic success. There are, however, several American surveys that measure only adult scores.

The Veteran's survey includes AFQT scores for 25-39 year old men tested between the ages of 19 and 26. Some of these men had additional schooling after their military service. The PSID includes sentence completion scores for men aged 25-64 who were no longer in school. The Malmo results suggest that these post-educational tests should explain more of the variance in economic success than do adolescent tests from surveys like Talent and Kalamazoo. Yet no such pattern emerges. For Occupation, eta^2 was 0.143 for Veterans, 0.128 for PSID, 0.213 for Talent, and 0.154 for Kalamazoo. The bivariate regression coefficient was 0.[illegible]2 for Veterans, 0.50 for PSID, 0.77 for Talent, and 0.69 for Kalamazoo. For Income, the eta^2s were 0.081 for Veterans, 0.106 for PSID, 0.026 for Talent, and 0.111 for Kalamazoo. These comparisons are complicated by the fact that (1) the PSID sentence completion test is far less reliable than the other three tests, (2) the education distribution has been artificially truncated in the Veteran's sample and in Talent, (3) the PSID occupation metric is somewhat different from the others, and (4) the Talent earnings measure differs from the others and has less variance because of the youth of the Talent men. Thus it is not surprising that adult tests do not seem to be more associated with occupation and income than adolescent tests in these studies. This does not necessarily mean that adult tests do not have larger effects. More likely, testing men with different amounts of education increases the predictive power of the tests, but these increases are offset by other downward biases. In the Veteran's sample the downward bias comes from underrepresentation of men with extreme values on education, and perhaps also from the inclusion of tool functions in the test given to younger men. In the PSID it comes from an unreliable test.

I conclude that measures of adult ability probably have higher correlations with success than measures of adolescent ability, since adult tests incorporate the effects of differential education. The higher association is seen in the Malmo study but is masked in the American surveys, which do not provide comparable data for adolescents and adults.

(b) Adult Ability and Occupational Status

Having compared the relative importance of adolescent and adult tests, I turn to the relative importance of adult tests vs. other factors for occupational success.

Age Stability

Adult tests seem to have a stable relationship with men's occupational status between 25 and 64. In the PSID, for example, the correlations were 0.353 for men 25-34 in 1972, 0.326 for men 35-44, 0.399 for men 45-54, and 0.327 for men 55-64. There is no consistent upward or downward trend in these correlations over age groups. Fagerlind's results support this conclusion for men over 30.

Effects of Adult Tests on Occupation

Table 4.15 shows regressions of occupation on adult tests for the PSID 25-64 year olds and 30-34 year old veterans. Test scores explain 13 percent of the variance in occupational status for PSID men

Table 4.15

Regressions of Occupation on Adult Test Scores in Different Samples

Sample (Year of Occupation)		Coefficient for Test Score B	Coefficient for Test Score Beta	% Reduction in Test Score Coefficient	Other Variables Controlled: TEST SCORE^2	ED	EDPASTHS	BA	VOCTRAIN	WORKEXP	JOB TENURE	MEASURED BKG	R^2
PSID (1971)[1] N = 1774	1)	.50310	.358	--									.128
	2)	.33391	.238	33.6	X							(2)	.226
	3)	.09915	.071	80.3	X	X	X	X	X			(2)	.458
	4)	.09855	.070	80.4	X	X	X	X	X	X	X	(2)	.458
NORC Veterans (1964) N = 803	5)	.705	.431	--									.186
	6)	.603	.369	14.5								(3)	.244
	7)	.230	.140	67.4		X	X	X				(3)	.448

All coefficients are larger than twice their standard error.

Variable identifications not seen on previous tables are: VOCTRAIN = Vocational Training; JOB TENURE = Years on Current Job.

1. PSID test converted to an S.D. = 15 for these comparisons.

(2) Indicates controls for White, Father's Education, Father's Occupation, Father White Collar, Father Foreign, Father Absent, Siblings, Non-Farm Upbringing, Non-South Upbringing, Father's Occupation^2, and Siblings^2.

(3) Indicates controls for White, Father's Education, Father's Occupation, No Male Head, Non-Farm Upbringing, and Non-South Upbringing.

and 19 percent for veterans. Controlling measured family background reduces the regression coefficient by 14 percent in the Veteran's survey and 34 percent in the PSID.

Differences in education explain most of the test score effects on occupation. Controlling education reduces the test score coefficient 80 percent in the PSID and 67 percent in the Veteran's survey. Some men returned to school after military service in the Veteran's sample. The regressions in Table 4.15 are only comparable if education prior to military service affects AFQT, and AFQT does not affect postmilitary schooling. 14/

Table 4.15 suggests that the effects of adult tests on occupation, net of differences in education, are quite small. A 15 point increase in adult scores boosts occupational status only 1 to 4 points when men have the same amount of schooling. These results confirm what we saw earlier with adolescent tests, namely that while cognitive skills help a man get through school, they do not boost his occupational status much if he does not get through school. They also suggest that if employers view cognitive skills as essential for high status occupations, they impose this requirement by the relatively inefficient device of requiring educational credentials. Alternatively, employers may not see cognitive ability as a prerequisite for high

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14/ See Griliches and Mason (1972). If AFQT affects postmilitary schooling, then the regressions in Table 4.15 will understate the effects of AFQT on occupation.

status occupations. They may believe that credentialed individuals are capable of learning more on the job, or have more suitable attitudes and values.

The findings in Table 4.15 are not modified by considering interactions among the determinants of occupation. None of the multiplicitive interactions between Race, Father's Education, Father's Occupation, Father Absent, Siblings, Test Score, Education, and Experience was significant in the PSID. Nor did the Test Score coefficient differ significantly for subsamples divided by respondent's race or by having white collar, blue collar, or farm fathers. The coefficient for AFQT is significantly higher for men with white collar fathers than for men with blue collar fathers in the Veterans sample. The AFQT coefficient for men with blue collar fathers is, in turn, larger than that for men with farm fathers, but not significantly larger. Not too much weight should be put on these interactions since they do not appear in the PSID or for adolescent test scores.

(c) Adult Ability and Earnings

Age Stability

The PSID sentence completion test correlated 0.285, 0.328, 0.407, and 0.331 with the earnings of men aged 25-34, 35-44, 45-54, and 55-64. The correlation increases as men approach their middle 50s and then decreases slightly for the oldest cohort. However, the decrease for the oldest cohort is not statistically significant. Therefore, I conclude that the adult test score-earnings relationship probably rises as men approach their middle 50s and then remains fairly stable. This conclusion must be very tentative, however, since it rests on the limited evidence of a single set of data.

Effects of Adult Ability on Earnings

Table 4.16 presents regressions of Ln Earnings on adult tests for 25-64 year old PSID men and 30-34 year old veterans. The bivariate association of adult test scores and Ln Earnings is substantial. Each one point increase in test performance is associated with a 1.8 percent increase in earnings in PSID and a 1.2 percent increase for Veterans. Controlling measured background reduces both coefficients by about a quarter. If education did not affect adult tests, this would suggest that 73 to 75 percent of the bivariate association of test scores and Ln Earnings was causal. But since education almost certainly does affect adult tests, the way to estimate the effect of adult tests on earnings is to control education along with background. Controlling education and background reduces the test score coefficient by 44 percent for veterans and 58 percent in PSID. With education and background controlled, a one point test score advantage increases earnings by about 0.7 percent in both surveys. Controlling Work Experience and Occupation reduces this to about 0.6 percent in both surveys.

Table 4.16

Regressions of Ln Earnings on Adult Tests in Different Samples

Sample (Year of Earnings)		Coefficient for Test Score B	Coefficient for Test Score Beta	% Reduction in Test Score Coefficient	ED	EDPASTHS	BA	VOCTRAIN	WORKEXP	WORKEXP^2	JOB TENURE	EXP INTERACT	OC	MEASURED BKG	BKG INTERACT	R^2
					Other Variables Controlled											
PSID (1971)[1] N = 1774	1)	.01773	.353	--												.125
	2)	.01300	.259	26.7										(2)		.172
	3)	.01292	.257	27.1										(2)	(3)	.178
	4)	.00753	.150	57.5	X	X	X	X						(2)	(3)	.255
	5)	.00670	.134	62.2	X	X	X	X	X	X				(2)	(3)	.298
	6)	.00663	.132	62.6	X	X	X	X	X	X	X			(2)	(3)	.330
	7)	.00662	.132	62.7	X	X	X	X	X	X	X	(4)		(2)	(3)	.334
	8)	.00595	.118	66.4	X	X	X	X	X	X	X	(4)	X	(2)	(3)	.353
NORC Veterans (1964) N = 803	9)	.01224	.351	--												.123
	10)	.00920	.264	24.8										(5)		.185
	11)	.00687	.197	43.9	X									(5)		.196
	12)	.00668	.192	45.4	X				X					(5)		.202
	13)	.00563	.162	54.0	X				X				X	(5)		.238

All coefficients are larger than twice their standard error.

Notes for Table 4.16

1. PSID test converted to SD = 15 for these comparisons.

(2) Indicates controls for White, Father's Education, Father's Occupation, Father White Collar, Father Foreign, Father Absent, Siblings, Non-Farm Upbringing, Non-South Upbringing, Father's Education^2, and Father's Occupation^2.

(3) Indicates controls for Father's Occupation * Father's Education, White * Siblings, Siblings * Father's Education.

(4) Indicates controls for Experience * Father Absent, Experience * Father's Education.

(5) Indicates controls for White, Father's Education, Father's Occupation, Male Head Absent, Non-Farm Upbringing, and Non-South Upbringing.

Both of our samples show that adult tests affect earnings after background, education, experience, and occupation variables are controlled. A 15 point increase in test scores results in a 9 percent increase in PSID and an 8 percent increase for veterans. Neither of these samples fully controls for family background, however.

To investigate whether the effects of adult ability on Ln Earnings change with men's age, I compared regressions of Ln Earnings on Test Scores with background, education,and experience controlled for the four PSID age cohorts. The standardized coefficient for test score was 0.129, 0.061, 0.200, and 0.161 for men aged 25-34, 35-44, 45-54, and 55-64. These coefficients do not appear to change systematically.

I also investigated whether high scorers earn more because they are better able to convert their education into higher earnings. This would imply a significant test score by education interaction. This interaction is less than twice its standard error in both the PSID and the Veteran's survey. Examination of over 20 multiplicative interactions in the PSID and Veterans surveys shows no consistent pattern of interactions.

The findings on adults tests lead me to several conclusions:

First, adult tests probably have higher associations with adult success than adolescent tests , since they capture differences in length of schooling along with adolescent ability.

Second, the correlation between adult tests and adult occupational status appears to remain constant as men become older, but the correlation between adult tests and adult earnings does not stabilize until they reach at least 30.

Third, after controlling for causally prior traits that inflate the association between adult tests and adult success, adult test scores have trivial effects on men's occupational status. They have larger effects on earnings. As noted earlier, a 15 point increase in adult test scores resulted in about a 9 percent increase in men's annual earnings. While this is a substantial amount, it is not large relative to the gap between the rich and the poor in general. The difference between the earnings of the best and worst paid fifths of male earners is about 600 percent. This implies that changing men's test scores without changing the social structure will not take us very far towards reducing economic inequalities.

General Conclusions

The analyses in this chapter support the following general conclusions:

1. Standardized tests of high school academic achievement predicts adult economic success better than any of the 30 "ability" tests I examined. High school academic achievement correlates quite highly with tests of verbal or quantitative ability, but academic achievement predicts later success better than either verbal or quantitative ability does. Skills that do not correlate highly with academic achievement correlate poorly both with one another and with later educational and economic success.

2. Ninth grade academic achievement predicts eventual success as accurately as 12th grade academic achievement. This suggests that it is not achievement per se that affects later success, but the motivations and aptitudes that lead to achievement. Since aptitude tests do not predict success as well as achievement tests, the predictive power of the achievement tests probably derives in part from its relationship to stable motivational factors. Alternatively, achievement tests could capture aptitudes not measured by conventional aptitude tests. The distinction between "aptitude" and "motivation" is, however, difficult to operationalize.

3. Students whose adolescent ability differs by one standard deviation typically differ by at least 1.5 years on education, 8 to 12 points on occupational status, about 7 percent on earnings at age 28, and about 15 percent on earnings after 35 in our samples. The relationship of test scores to earnings appears to rise until men reach about 35 and to remain stable for men between 35 and 55. It could fall after 55.

4. The relationship of ability to adult success depends partly on the fact that both are affected by family background. Demographic background measures do not capture the full effect of family background on either test performance or adult success, as we discover when we look at sibling data. Using sibling data, the apparant effects of test performance are reduced by 32-43 percent for education, a roughly similar amount for occupational status, but only about 25 percent for earnings at 28. The estimated effect of test score on earnings in maturity is actually larger when we control all aspects of family background than when we omit such controls, but this is probably a sampling anomaly.

5. Adolescent ability affects adult occupational status primarily by affecting education. This is less true for earnings. If two men differ by one standard deviation on Talent's composite but do not differ on education, Appendix H shows that their Duncan scores at 28 typically differ by only 3.7 points. Their earnings at 28 will differ by about four percent. Among Kalamazoo 35-59 year olds, the occupational difference is 3.4 points and the earnings difference is 14 percent.

6. The relationship of adolescent ability to educational attainment, occupational status, or earnings does not appear to have changed over time for men born between 1900 and 1940. The data on which this conclusion is based are far from conclusive, but are somewhat stronger for education than for occupation or earnings.

7. I found no consistent evidence that students with high test scores derived more economic benefit from additional education than did students with average or below average test scores.

CHAPTER 5: EFFECTS OF NONCOGNITIVE TRAITS

by Peter Mueser

Introduction

Common sense tells us that individuals possess characteristics not measured by cognitive tests, and that these characteristics affect their social and economic success. Most people assume, for example, that individuals with "ambition," "good attitudes," "high aspirations," or "good judgment" are more likely to succeed than individuals with the same cognitive ability and family background who lack these characteristics. The widespread acceptance of this belief is reflected in the importance that employers and college admissions committees place on personal interviews, letters of recommendation, and other personal evaluations, even when cognitive tests and other measures of ability are available.

Past Research

Few studies have attempted to measure the importance of non-cognitive factors in determining life success.

Crockett (1962) argued that individual motivation "may play a key role in occupational mobility of persons sharing broadly similar opportunity." He presented data that showed a positive relationship between socio-economic mobility and an individual's score on "need for achievement" as determined by his response to the Thematic Apperception Test (TAT). When Duncan, Featherman, and Duncan (1972) reanalyzed Crockett's data, they found that need for achievement had a small direct effect on occupational status (standardized coefficient 0.12) after controlling father's occupation and respondent's education. However, since the data were not longitudinal, they could not rule out the possibility that the respondent's occupational status affected his TAT responses rather than the other way around.

Rosen (1971) also found a positive relationship between his measure of individual "achievement syndrome" and occupational mobility after controlling for background characteristics. Again, however, the data were not longitudinal.

Elder (1968) used longitudinal data to consider the influence of the noncognitive traits of high school students on eventual educational attainment and occupational status. He found that the TAT measure of need for achievement administered in high school had a negligible effect. He also looked at a second measure of "motivation" based on observers' ratings of students' behavior in high school This measure had appreciable effects on educational and occupational attainment (standardized coefficients 0.236 and 0.222, controlling I.Q. and social class). Unfortunately, Elder's sample was small (N=65), all white, and drawn entirely from Oakland, California, so it is unclear whether his findings could be generalized.

Featherman (1972) used longitudinal data to examine the effects of positive orientation to work, materialistic orientation, and perception of personal achievements at the age of 30 on men's occupational and economic success 3 to 10 years later. After controlling for individual background characteristics, education, and initial occupational status and income, he found that work orientation had a small effect on occupational status three years later (standardized coefficient 0.063), but no other statistically significant effects. Materialistic orientation affected income 3 and 10 years later (standardized coefficients 0.052 and 0.071), as did subjective achievement (coefficients 0.157 and 0.083).

Featherman suggested that if motivation were measured earlier it would be more important. In order to test this, he estimated a model which treated personality measures at age 30 as indicators of stable traits formed in adolescence. This model did not suggest that adolescent traits of this sort were of critical importance.

Sewell and Hauser (1975) analyzed the effects of several social-psychological factors on Wisconsin high school seniors' eventual educational attainment, occupational status, and earnings up to 10 years later. They found that after controlling for background characteristics and academic ability, students' high school grades, influence from teachers, peers and parents, and students' own educational and occupational aspirations all influenced years of education. These factors also affected occupational status and earnings, although their effects on earnings were small.

While Sewell and Hauser's analyses indicate that noncognitive traits of high school students influence later success, they leave unanswered questions about the ways in which these measures affect outcomes. Sewell and Hauser suggested that the encouragement a student receives and his level of aspirations affect his educational attainment and economic success, but such measures may merely reflect other underlying motivational characteristics.

Andrisani and Nestel (1976) found that "internal control" -- the extent to which an individual believes success is determined by personal initiative rather than external events -- had a positive relationship to occupational status and earnings for NLS men 45 to 59. They also found that this measure predicted change in earnings over the two years following the initial survey and that change in earnings affected the measure. This is consistent with theory, which suggests that internal control should affect success (see Rotter, 1966). But the direction of causation is still unclear, even in this longitudinal data. Individuals may believe they can control their lives because they face favorable circumstances, or because they possess other unmeasured characteristics that facilitate success. Nonetheless, Andrisani and Nestel's findings suggest that some aspect of personality may influence earnings.

Numerous studies have examined the relationship between personality and job success for individuals in restricted occupational groupings.[1] However, since samples are usually limited to a single occupational grouping (e.g., salesmen, managers), and often to a single company, it is difficult to determine whether relationships found in such limited, self-selected populations apply to representative samples. The personality measures also differ across studies, so results for different occupational groupings cannot easily be pieced together. Even if results were comparable within a wide range of occupations, they would not tell us anything about the effects of personality on occupational choice or selection. Therefore, these studies are of little help in determining the extent to which personality predicts success for the population in general.

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[1] See Korman (1968) for a review of longitudinal studies that relate managerial success to individual traits. Brenner (1968) related teachers' ratings of high school students with supervisors' ratings in a later job for production employees.

Data and Methods

The Talent and Kalamazoo surveys permit a more comprehensive investigation of the effects of adolescent noncognitive traits on subsequent social and economic success than any data available heretofore. The Talent survey provides personality ratings based on student self-reports, numerous details on student behavior in high school, students' perceptions of others' attitudes towards them, and personal aspirations and plans. Talent measured educational attainment and economic success 12 years after the initial survey. Kalamazoo teachers rated 10th graders on a variety of character traits. Olneck contacted these men when they were 35-59 to determine their educational attainment, and economic success. Both surveys provide measures of background characteristics and cognitive ability. Since Olneck surveyed brothers, one can also control whatever unmeasured family characteristics brothers have in common when analyzing his data.

Given the paucity of empirical evidence supporting any single theory, my analyses of these data are exploratory. I examine a large number of noncognitive variables in an effort to identify those which have predictive power. My first concern is to answer the global question of how important noncognitive traits are in the status attainment process, since this class of variables has been ignored in most stratification research. I also hope to draw conclusions about the nature of those traits that are important, and thus to shed light on the mechanisms by which individuals succeed or fail in our society. Since my concern is with the possible causal role of noncognitive traits, my analyses focus on effects after controlling for measured background characteristics.

My analyses are divided into four sections. The first examines the importance of 10 self-assessed personality traits. Project Talent derived these 10 measures from students' responses to the high school questionnaire. Since responses to individual questions are not available, these analyses hinge on the validity of the scales devised by Talent researchers.

The second section considers more than 60 questions on the Talent questionnaire relating to student activities, behavior, and attitudes. I treat these as proxies for students' noncognitive traits, and use principal component analysis to search for factors that have consistent effects on later success.

In the third section, I consider the combined effects of all the noncognitive traits measured in Project Talent in a model which also includes students' perceptions of encouragement, as well as students' explicit preferences and plans.

In the fourth section, I use the Kalamazoo survey to determine the importance of teacher ratings of student character on students' later success. Since Kalamazoo respondents were followed up when they were 35 to 59, comparisons of this analysis with Talent allow us to infer whether effects found for 28-year-old men in the Talent survey persist in later years.

1. Personality Self-Assessments

The Personality Self-Assessments are based on questions that require respondents to make judgments concerning their own actions, preferences, or the way others view them. Talent researchers grouped together statements which they thought described similar types of behavior to form the composites.

Table 5.1 lists these composites and selected items from each. Items differ in generality. For example, items for sociability range from "I prefer reading a good book to going out with friends" (weighted negatively) to simply "I am friendly" (weighted positively). Most items require the respondent to characterize his behavior as if it were relatively stable over time and across situations. An individual who did not believe in such stable traits could either omit the question or give the most noncommittal response. In either case, he would end up with a low score on the scale. It therefore seems fair to assume that the composites measure the extent to which the respondent believes he possesses a given set of traits.

With the possible exception of Impulsiveness, the composites measure perceived conformity to socially acceptable patterns of behavior. In a factor analysis using a Talent sample which differed from ours, Lohnes (1966, Chapter 5) found that all the composites, excluding

Table 5.1 Selected Questions from the 10 Talent Personality Self-Assessment Scales a/

Regarding the things I do and the way I do them, this statement describes me

A. extremely well
B. quite well
C. fairly well
D. slightly
E. not very well

An item is marked "+" when Talent scored options A or B as 1 and options C, D, and E as 0. The item is marked "-" when Talent scored options D and E as 1 and A, B, and C as 0. Scores on a scale are found by summing the scores on the items included in this scale. Thus scores range from zero to the number of items in the scale.

Sociability (12 items)

(+) People seem to think I make new friends more quickly than most people do.
(-) I prefer reading a good book to going out with friends.
(+) I am friendly.

Social sensitivity (9 items)
(+) I seem to know how other people will feel about things.
(+) People consider me a sympathetic listener.
(+) I am sympathetic.

Impulsiveness (9 items)
(+) I like to do things on the spur of the moment.
(+) I am impulsive.
(-) It takes me quite a while to come to a decision.

Vigor (7 items)
(+) I can work or play outdoors for hours without getting tired.
(+) I am energetic.

Calmness (9 items)
(-) People seem to think I get angry easily.
(+) I am even-tempered.
(+) I am usually self-controlled.

Tidiness (11 items)
(+) I am never sloppy in my personal appearance.
(+) Before I start a task, I spend some time getting it organized.
(+) I am neat.

Table 5.1 (continued)

Culture (10 items)
- (+) I enjoy beautiful things.
- (+) I take part in the cultural activities in my community.
- (+) I am refined.

Leadership (5 items)
- (+) I am the leader in my group.
- (+) I am influential.
- (+) I have held a lot of elected offices.
- (+) People naturally follow my lead.
- (+) I like to make decisions.

Self-Confidence (12 items)
- (+) I am confident.
- (+) I'd enjoy speaking to a club group on a subject I know well.
- (-) Being around strangers makes me ill-at-ease.

Mature Personality (24 items)
- (+) I make good use of all my time
- (+) I work fast and get a lot done.
- (+) It bothers me to leave a task half done.
- (+) I do my job, even when I don't like it.
- (+) I do things the best I know how, even if no one checks up on me.
- (+) I am dependable.
- (+) I am reliable.

a/ For a complete list of questions which comprise the 10 scales see The Project Talent Data Bank: A Handbook (1972), pp. 38-42.

Impulsivness, loaded on a single factor. He suggested that these composites were largely measuring an individual's need to conform. I coded all these composites so that the "approved" response led to a higher score than the "deviant" response. The correlations among these nine composites range from 0.28 to 0.62.

While "Impulsivness" is not usually considered a socially acceptable trait, this label may not be entirely appropriate to the items included under it. One might, for example, argue that the items in this scale really measure "decisiveness" rather than "impulsiveness," and that "decisiveness" is a socially desirable characteristic. Whether for this or other reasons, Impulsivness correlates positively with all the other composites (r = 0.11 to 0.25). It also correlates positively with later success. I therefore retained the original coding rather than transforming the scale into a measure of "nonimpulsiveness."

Responses to the 108 separate questions are not available for analysis, so it is not possible to determine whether important information was lost in constructing the composites. It is, however, possible to determine whether the effects of these composites deviate from linearity. I regressed the respondent's education, occupational status, and hourly wage on a quadratic function of each of the ten composites. In 8 of the 30 regressions, the deviation from linearity was statistically significant. All 8 of these regressions indicated that a one-point change had more effect at the top of the scale than at the bottom. This was not due to a "ceiling" effect, since the means of the composites are very close to the midpoints on the scale. But no composite had significant nonlinear effects on all three dependent variables. I therefore decided not to rescale the composites, but instead to include squared terms in my analyses to capture non-linear effects.

Determinants of Self-Assessed Personality Traits

The five basic background variables used in Appendix H (White, Father's Occupation, Father's Education, Father Absent, Siblings) explain less than three percent of the variance in the self-assessed personality traits (see Column 1 of table 5.2).[2] This remains true even when one adds non-linear and interaction terms (see Column 2). I also examined the effects of adding nine additional background characteristics, including Family Income, Mother's Education, Mother's Occupation and Talent's composite Socioeconomic Index. Because of missing data, adding these variables decreases the sample size to 663. Comparing Column 3 to Column 2 shows that this restriction changes very little. Comparing Column 4 to Column 3 shows that even with 14 background variables, their squares, and selected interactions, $\bar{R}^2$ never exceeds 0.06. Of the additional variables. only the Socioeconomic Index had a consistent effect across the composites. Column 5 indicates

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[2] The proportion of variance explained is corrected for sample size. This is adjusted R^2 or $\overline{R^2}$ given by the formula:

$$\bar{R}^2 = R^2 - \frac{k-1}{N-k}(1-R^2)$$

where N is the number of cases, k is the number of dependent variables (including constant), and R^2 is the proportion of variance actually explained. This $\overline{R^2}$ is an estimator of the proportion of variance which would be explained by the dependent variables in an infinite sample. Although this measure is biased, the bias is small, never greater than 0.096/N (see Barten, A.P., "Note on Unbiased Estimation of the Squared Multiple Correlation Coefficient," Statistica Neerlandica 16, pp. 151-163 (1962). In contrast, R^2 is always an upwardly biased estimate. For a given sample size, the bias is a linear function of the number of dependent variables. I restrict discussion to $\overline{R^2}$.

Table 5.2 Proportion of Variance Explained/ $(\bar{R}^2)$ in Self-Assessed Personality Measures by Background Characteristics: Talent Males

	Primary Sample (N=1048)		Restricted Sample (N=663)		
	(1)	(2)	(3)	(4)	(5)
Sociability	.011	.006	.004	.032	.010
Social Sensitivity	.020	.027	.015	.016	.027
Impulsiveness	.002	.007	.031	.037	.038
Vigor	.005	.013	.013	.016	.030
Calmness	.021	.023	.009	.013	.013
Tidiness	.003	.004	.003	-.011	.011
Culture	.012	.019	.019	.021	.029
Leadership	.019	.022	.025	.059	.041
Self Confidence	.016	.020	.018	.024	.017
Mature Personality	.012	.014	.001	.024	.006
Controls					
5 - Basic Background Characteristics	X	X	X	X	X
Squares, Interactions		X	X	X	X
Additional Variables					
Socioeconomic Index				X	X
8 Other Background				X	
Squares, Selected Interactions				X	

Table 5.2 (continued)

Notes:

Sample The sample is restricted to 1427 males who filled out questionnaires in 1960 as part of the Project Talent Survey of eleventh graders, and who also responded to a follow up questionnaire in 1972 (see Appendix H for complete description of selection process). In contrast to other analyses presented in this book, I have chosen not to restrict my sample to civilian nonstudents. 1.8 percent of the 1427 individuals in the sample report that they are full time students, and also reported the number of years of education they had completed. These individuals average 16.1 years of reported schooling. Only two of the students report occupation and hourly earnings. 0.5 percent of the sample report they are officers in the military, and I assigned these individuals a Duncan score of 58 in accordance with a coding scheme devised by Marsha Brown. Although a Duncan score exists for enlisted men, no individuals have this code, probably due to an error in the coding scheme.

The Primary Sample consists of the 1048 individuals with complete data on the five basic background variables, four dependent variables, and self-assessed personality traits. The Restricted Sample is a subset of these individuals who also have complete data on eight additional background controls (N=663).

Controls Basic background variables are White, Father's Education, Father's Occupation, Father Absent and Siblings. Squares of the 5 background variables are Father's Education^2, Father's Occupation^2, and Siblings^2. Interactions of the five basic background variables include all 15 possible two-way interactions. Eight additional controls include Family Income, Mother's Education, Mother Housewife, Mother's Occupation, Father Supervises, Books in the Home, Non-South Upbringing and Non-Farm Upbringing. Squares are computed for all nondichotomous variables. Interactions include all interactions involving the Socioeconomic Index, Mother's Education, and Family Income.

Variables White, Father's Education, Father's Occupation, Father Absent and Siblings are defined in Appendix H, which describes the Talent Sample.

Socioeconomic Index: Constructed by Talent researchers from student responses to nine questions concerning family characteristics. Questions ascertained family income, books in the family's home, mother's education, father's education, father's occupation, value of the family's home, and three measures relating to number of family appliances, televisions, and whether the respondent had his own room and desk. Construction of the index is fully described in The Project Talent Data Bank: A Handbook (1972).

Family Income: Coded from student categorized responses "less than 3,000" (1750); "3,000 to 5,999" (4500); "6000 to 8,999" (7500); "9,000 to 11,999" (10500); "12,000 or more" (19000); "I can't estimate" (missing).

Mother's Education: Coded from student responses using the same code as Father's Education

Mother Housewife and Mother's Occupation: Mother Housewife was coded 1 if student reported that his mother was a housewife or if he did not report

Table 5.2 (continued)

an occupation for her, and 0 otherwise. If the student reported an occupation for her, Mother's Occupation is coded into Duncan scores as is Father's Occupation. Otherwise Mother's Occupation was assigned the mean.

Father Supervises: number of employees the student reports that his father supervises coded from categories "no [does not supervise]" (0); "yes, a few people (up to 4 or 5)" (5); "yes, many people (from 6 to 19)" (12); "yes, very many people (20 to 49)" (34); "yes, 50 or more people" (80); "I don't know" (missing).

Books in Home: number of books a student reports are in his home, coded from his responses in categories "none or very few (0-10)"(5); "a few books (11-25)" (18); "One bookcase full (26-100)" (63); "two bookcases full (101-250" (176); "three or four bookcases full (256-500" (376); "a room full - a library (501 or more)" (800).

Non-South Upbringing: Coded from region of high school

Non-Farm Upbringing: Coded from student's report of father's occupation.

that dropping the other 8 "supplementary" background measures, their squares, and interactions typically raises $\bar{R}^2$ (by increasing the degrees of freedom). It is clear that the five basic background measures plus the Socioeconomic Index account for almost all background effects on self-reports.

Effects on Education

Table 5.3 presents effects of the self-report composites on Years of Education. Column 1 presents zero-order correlations. Columns 2 through 6 present standardized coefficients with various controls. Note, however, that none of the regressions includes more than one personality composite at a time. Sample A covers all respondents with basic background data. Sample B includes only those with additional background characteristics. Comparison of Columns 3 and 4 for Sample B indicates that the additional background controls hardly alter the coefficients, so I will ignore Sample B.

Column 1 of Table 5.4 presents regressions of Education on all ten self-assessed personality measures at once, controlling background characteristics. Since the ten measures are not conceptually distinct, standardized coefficients in a regression which enters all of them may be misleading. The signs and relative sizes of coefficients can, however, be used to infer possible causal relationships between personality measures. In this regression, Leadership and Mature Personality have statistically significant positive effects while Sociability has a negative effect. Thus while Sociability is associated with greater educational attainment (see table 5.3), this is only because it is correlated with the Leadership and Mature Personality composites. If

Table 5.3 Regression of Education on Self-Assessed Personality Measures
Taken One at a Time: Talent Males

	r	Standardized Coefficients				
	(1)	(2)	(3)	(4)	(5)	(6)
Sociability						
Sample A	.070	[.026]	[.026]		[.043]	[.022]
Sample B	.044		[.002]	[-.001]	[.018]	[-.002]
Social Sensitivity						
Sample A	.183	.096	.098		.060	[.041]
Sample B	.142		.072	[.063]	[.037]	[.023]
Impulsiveness						
Sample A	.044	[-.005]	[-.003]		[.002]	[-.004]
Sample B	.063		[.004]	[.013]	[.022]	[.027]
Vigor						
Sample A	.145	.089	.086		.064	[.039]
Sample B	.108		.088	.087	.067	[.041]
Calmness						
Sample A	.192	.133	.112		[.048]	[.030]
Sample B	.134		.075	[.058]	[.010]	[-.010]
Tidiness						
Sample A	.149	.092	.098		.064	[.033]
Sample B	.105		.079	.091	.069	[.037]
Culture						
Sample A	.197	.114	.119		.104	.077
Sample B	.166		.094	.089	.087	[.060]
Leadership						
Sample A	.201	.140	.144		.101	.074
Sample B	.180		.113	.108	.086	.059
Self-Confidence						
Sample A	.121	.052	[.052]		[-.003]	[-.021]
Sample B	.100		[.047]	[.043]	[-.003]	[-.029]

Table 5.3 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
Mature Personality						
Sample A	.228	.162	.165		.087	[.046]
Sample B	.216		.172	.164	.092	[.043]
Controls						
6 Background Characteristics						
Basic Background (11 terms)		X	X	X	X	X
Other Squares, Interactions			X	X	a)	a)
Additional Variables, Squares and Interactions				X	a)	a)
Test Score (quadratic)					X	X
Grades						X

a) Controlled in Sample B only.

Notes:

Sample A: Individuals with complete data on all self-assessed personality measures, as well as Respondent White, Father's Education Father's Occupation, No Male Head, Siblings, Socioeconomic Index, and Test Score (N=1002).

Sample B: Individuals in Sample A with complete data on Father Supervises, Family Income, Books in Home, Mother's Education, Mother's Occupation, Mother Housewife, Non-South Upbringing, and Father Farmer (N=663).

Controls

Basic Background controls include Respondent White, Father's Education, Father's Occupation, Father Absent, Siblings, Talent's Socioeconomic Index^2, Socioeconomic Index X Siblings, White X Father Absent, Father's Education X Siblings, and Father Absent X Siblings. These 11 terms are used as background controls in most of the analyses which follow. See note below for explanation.

Other Squares and Interactions include Father's Education^2, Father's Occupation^2, Siblings^2, as well as the 11 interactions between linear background terms not entered above.

Nine additional background factors are Father Supervises, Family Income, Books in Home, Mother's Education, Mother's Occupation, Mother Housewife, Non-South Upbringing, and Non-Farm Upbringing. I entered squares for all nondichotomous variables and interactions involving Family Income, Socioeconomic Index, and Mother's Education.

Controls for Test Score include both a linear and square term.

The control for Grades is the simple linear variable (see below).

Variables

Detailed definitions of Test Score and Grades are given in Appendix H. Other variables are defined in notes to Table 5.2.

Notes on Controls

In analyses that follow, I control background with 11 terms based on 6 personal characteristics. These controls were chosen as follows: I included Respondent White, Father's Education, Father's Occupation, Father Absent, and Siblings on *a priori* grounds. I entered these five variables in regressions predicting Years of Education, Occupation, Hourly Earnings and Ln Hourly Earnings, and then tested to see if Talent's Socioeconomic Index, any squares of the variables, or any of the 30 interactions among them added significantly to the proportion of variance explained. After those which were significant had entered, I controlled the Test Score (and Test Score^2 in the regression predicting Years of Education), and again tested to see if any of the unentered background terms increased the proportion of variance explained. I used separate samples for regressions predicting Education (N=1039), Occupation (N=931) and Hourly Earnings (N=909) so as to uncover any effects which might be present.

The Socioeconomic Index, Socioeconomic Index^2, Socioeconomic Index X Siblings, White Father Absent, Father's Education X Siblings and Father Absent X Siblings were significant in at least one regression. I then regressed dependent variables on these 11 terms, and tested the unentered terms. None was significant. Controlling these 11 factors therefore assures us that results will not be biased by nonlinearities or interactions involving our 6 background measures.

Test Score is controlled using both a linear and a square term in regressions predicting Years of Education. It was usually significant in such regressions and had a consistently positive coefficient. The square for Test Score was never significant in regressions predicting other outcomes, so I controlled only the linear effect of Test Score in such regressions.

The square of Grades was not statistically significant in any regression after Test Score was controlled, so I have only controlled its linear effects.

Where I control for Education, I have used the three variables Years of Education, College Graduation, and Years of Graduate School, which allow for effects of Schooling to be nonlinear. These are described in Appendix H.

Table 5.4 Regressions of Education on Self-Assessed Personality Measures Controlling for Background and Intervening Factors: Talent Males

	Standardized Coefficients					
	(1)	(1a)[a/]	(2)	(2a)[a/]	(3)	(3a)[a/]
Sociability	-.077	-.070	[-.013]		[-.016]	
Social Sensitivity	[-.004]		[-.019]		[-.010]	
Impulsiveness	[-.042]		[-.030]		[-.026]	
Vigor	[.021]		[.020]		[.018]	
Calmness	[.028]		[-.017]		[-.006]	
Tidiness	[-.017]		[-.003]		[-.010]	
Culture	[.037]		.082	.075	.077	.067
Leadership	.105	.105	.084	.070	.071	.063
Self-Confidence	[-.016]		[-.051]		[-.052]	-.055
Mature Personality	.122	.142	[.042]		[.008]	
Controls						
Background	X	X	X	X	X	X
Test Score			X	X	X	X
Grades					X	X
$\bar{R}^2$ Controls only	.248	.248	.420	.420	.441	.441
$\bar{R}^2$ with 10 Self Assessments	.279	.281	.433	.443	.446	.449
Significance of increment in R^2 from Self-Assessments	$p < .01$		$p < .01$		$p < .05$	
Combined Coefficient[b/] of All Significant Self-Assessments		.189		.123		.104

Table 5.4 (continued)

Notes:

Sample Restricted as Sample A, Table 5.3 (N=1002).

a/ In columns 1a, 2a and 3a measures of self-assessed personality traits were added in the order of their contribution to explained variance until no unentered variable increased $\overline{R}^2$ significantly.

b/ This is the standardized coefficient for the variable constructed by multiplying each component variable by its unstandardized coefficient and summing. It can be computed from the coefficients for the individual variables as

$$\sum_{j=1}^{N} \sum_{i=1}^{N} b_i b_j r_{ij}$$

where b_i and b_j are the standardized coefficient for the i^{th} and j^{th} variables, r_{ij} is the correlation between them, and N is the number of variables to be combined.

these composites really measure what their labels imply, we might claim that sociability increases with leadership and maturity, but that at any given level of leadership and maturity the more sociable students are also more likely to drop out.

In order to compare the relative importance of family background, ability, and self-assessed personality, I created a variable that combined all the significant composites into a single "super-composite." I constructed this new variable by multiplying each significant trait by its unstandardized coefficient and summing. The bottom row of Table 5.4 shows results with various controls. With only background controlled, the standardized coefficient of this new composite is 0.189. Controlling academic ability (equation 2a) reduces the coefficient of the personality composite by a third. This is because the positive effect of Mature Personality and the negative effect of Sociability both grow weaker. Those with "Mature Personality" but low "Sociability" (an atypical combination) get more education largely because they have high academic aptitude. If we also control for students' grades, the combined coefficient falls slightly further to 0.104.

These results imply that the relative importance of self-assessed personality traits to an individual's educational attainment depends on whether personality determines academic ability and high school grades or the other way around. If self-assessed traits precede ability, the combined unstandardized coefficient of 0.189 is the best measure of their importance. 35 percent of the effect can then be said to derive from the fact that personality traits influence test performance. Of the remaining influence, 15 percent works directly through high school grades. If we assume that self-assessed traits either follow or are codetermined with academic ability and grades, we must control ability and grades in order to estimate the true effect of self-assessed traits. The combined coefficient is then 0.104. I regard this as a more reasonable estimate than 0.189. By contrast, the coefficient for Test Score is 0.405 and the zero-order correlation between the Socioeconomic Index and Education is 0.456.

Effects on Occupational Status

Table 5.5 presents zero-order correlations and standardized coefficients of the separate self-assessments when we predict Occupation. Table 5.6 presents the coefficients for the measures taken together.

The pattern is similar to that when we predict Years of Education, but the effects are generally weaker. With background controlled, the combined standardized coefficient of all significant self-assessed measures is 0.159. When cognitive ability is controlled the coefficient declines to 0.119, far smaller than the coefficient for Test Score (0.373). After controlling high school grades, only Culture remains statistically significant, and it is only barely so. None of the self-assessments has a direct effect on Occupation after Education has been controlled.[3/] This suggests that among men with the same amount

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[3/] Columns 5 and 6 in Table 5.5 show that coefficients of measures are not significant when measures were considered separately in any regression predicting Occupation after Education is controlled. Coefficients were not significant in regressions that entered more than one measure, either.

Table 5.5 Regressions of Occupation on Self-Assessed Personality Measures Taken One at a Time: Talent Males

	r	Standardized Coefficient				
	(1)	(2)	(3)	(4)	(5)	(6)
Sociability	.065	[.030]	[.026]	[.012]	[.000]	[.003]
Social Sensitivity	.115	[.051]	[.012]	[-.004]	[-.019]	[-.007]
Impulsiveness	.020	[-.033]	[-.027]	[-.029]	[-.027]	[-.028]
Vigor	.122	.072	[.034]	[.017]	[.001]	[.013]
Calmness	.170	.112	[.053]	[.039]	[.024]	[.041]
Tidiness	.127	.092	[.056]	[.031]	[.015]	[.029]
Culture	.162	.104	.090	.066	[.032]	[.036]
Leadership	.168	.116	.081	[.056]	[.0242	[.035]
Self-Confidence	.137	.085	[.038]	[.023]	[.043]	[.061]
Mature Personality	.181	.130	.065	[.027]	[.006]	[.032]
Controls						
Background		X	X	X	X	X
Test Score			X	X	X	
Grades				X	X	
Education					X	X

Sample Individuals with complete data on background controls, as well as Test Score, Grades, Years of Education and Occupation (N=898).

Table 5.6 Regressions of Occupation on Self-Assessed Personality Measures Controlling Background and Intervening Factors: Talent Males

	Standardized Coefficients				
	(1)	(1a)	(2)	(2a)	(3)
Sociability	[-.051]		[-.013]		[-.014]
Social Sensitivity	[-.070]		-.084	-.078	[-.076]
Impulsiveness	[-.064]	-.071	[.050]		[-.045]
Vigor	[.012]		[-.004]		[.000]
Calmness	[.057]		[.022]		[.033]
Tidiness	[.011]		[.010]		[.007]
Culture	[.055]		.096	.105	.089
Leadership	.090	.087	.078	.071	[.064]
Self-Confidence	[.032]		[.006]		[.006]
Mature Personality	[.069]	.102	[.015]		[-.022]
Controls					
Background	X	X	X	X	X
Test Score			X	X	X
Grades					X
$\bar{R}^2$ Controls only	.142	.142	.255	.255	.271
$\bar{R}^2$ with Self-Assessments	.165	.164	.264	.267	.275
Significance of 10 Self-Assessments	p < .01		p < .05		p > .05
Combined Coefficient of Significant Self-Assessments		.159		.119	

Sample is as specified in Table 5.5 (N=898).

For explanation of column headings, see Table 5.4.

of education, prior personality self-assessments are not important in determining who will get a high status job. These adolescent personality traits must affect occupational status largely if not exclusively by affecting education.

Effects on Earnings

Table 5.7 presents effects of self-assessed personality on Hourly Earnings. The Leadership composite has the largest effect (0.202). With Leadership controlled none of the other self-assessments had a statistically significant coefficient.[4] Controlling for academic ability and grades decreases the coefficient of Leadership less than 10 percent, implying that only a small portion of the effect is due to its association with cognitive ability.

A one-standard deviation increase on the Leadership measure increases Hourly Earnings by about 45 cents. This return may seem modest when we consider that Hourly Earnings averaged $5.27, and that the standard deviation was $2.24. Nonetheless, the standardized effect is larger than that of any other adolescent trait Talent measured, including background and test score. The combined coefficient of the background characteristics (with no other controls) is 0.16. The combined coefficient of test score and grades is 0.115 with background and leadership controlled.

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[4] Since self-assessments other than Leadership never had significant coefficients with Leadership controlled, I have not presented the equations.

Table 5.7 Regression of Hourly Earnings on **Self-Assessed Personality Measures Taken One at a Time: Talent Males**

	r (1)	(2)	(3)	(4)	(5)	(6)
Sociability	.130	.116	.115	.111	.098	.100
Social Sensitivity	.136	.107	.095	.090	.075	.080
Impulsiveness	.028	[.013]	[.015]	[.014]	[.015]	[.015]
Vigor	.120	.104	.092	.087	.077	.082
Calmness	.127	.097	.082	.077	.067	.073
Tidiness	.127	.110	.099	.092	.080	.086
Culture	.109	.085	.082	.075	[.059]	[.061]
Leadership	.215	.202	.195	.191	.181	.183
Self-Confidence	.136	.111	.096	.092	.096	.102
Mature Personality	.156	.132	.114	.107	.105	.110
Controls						
Background		X	X	X	X	
Test Score			X	X	X	
Grades				X	X	
Education (nonlinear)					X	X
$\bar{R}^2$ for Controls only		.026	.038	.040	.065	.064
$\bar{R}^2$ with Leadership		.065	.073	.073	.094	.094

Sample Individuals with complete data on all background controls, as well as Test Score, Grades, Years of Education and Hourly Earnings (N=875).

Table 5.8 Regressions of Ln Hourly Earnings on **Self-Assessed Personality Measures Taken One at a Time: Talent Males**

		Standardized Coefficient				
	r (1)	(2)	(3)	(4)	(5)	(6)
Sociability	.108	.093	.093	.089	.076	.077
Social Sensitivity	.120	.091	.078	.074	[.059]	[.065]
Impulsiveness	.036	[.021]	[.024]	[.022]	[.023]	[.023]
Vigor	.109	.094	.081	.076	[.066]	.073
Calmness	.099	.072	[.053]	[.049]	[.040]	[.048]
Tidiness	.116	.099	.087	.081	.070	.076
Culture	.083	[.060]	[.057]	.050	.035	[.037]
Leadership	.161	.144	.135	.131	.123	.125
Self-Confidence	.130	.106	.089	.085	.088	.097
Mature Personality	.127	.105	.083	.076	.075	.085
Controls						
Background		X	X	X	X	
Test Score			X	X	X	
Grades				X	X	
Education					X	X
$\bar{R}^2$ **Controls Only**		.022	.038	.039	.061	.058
$\bar{R}^2$ **with Leadership**		.041	.054	.054	.073	.070

Sample as specified in Table 5.7 (N=875).

Only a small part of the effect of Leadership on earnings works through education. Controlling years of schooling, college graduation, and years in graduate school lowers the standardized coefficient for Leadership to 0.181. For comparison, the combined coefficient for the three education variables is 0.183.

The standardized effects of the personality self-assessments on Ln Hourly Earnings are smaller than on Hourly Earnings (see Table 5.8). Personality self-assessments evidently explain less of the variation near the bottom of the earnings distribution than near the top.[5]

Effects of Self-Assessed Personality Traits Summarized

Self-assessed personality traits have moderate effects on education if we assume that personality affects academic ability and high school grades but not vice versa. If, more reasonably, we assume that personality depends on ability and grades, the self-assessments do not have very large effects on education, especially in comparison to ability. The effects of self-assessed personality on occupational status at 28 follow the same pattern. Controlling for education reduces all personality self-assessments to statistical nonsignificance.

After Leadership is controlled, none of the other personality self-assessments has much effect on hourly wage at 28. But Leadership has a larger effect on Hourly Wage than on Education or Occupation.

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[5] Coefficients could also be lower because effects on Ln Hourly Earnings conformed poorly to the linear model. However, this is unlikely, since effects of the self-assessments were seldom significantly nonlinear in regressions predicting Ln Hourly Earnings.

While zero-order correlations with hourly wage are not much higher than with education or occupational status, far less of the correlation is explained by background factors, cognitive ability, or grades. Leadership also affects earnings independently of education.

Students who characterize themselves as vigorous, calm or tidy apparently have no particular advantage in later achievements after other self-assessed characteristics are controlled. Those who are more socially oriented (as reflected in the Sociability and Social Sensitivity self-assessments) obtain less schooling and lower status occupations when the other self-assessed traits (in particular Leadership and Mature Personality) are controlled. This supports the notion that socially oriented individuals are less interested than others in academic work. Despite their lower educational and occupational attainments, sociable students' wages at age 28 are not appreciably lower, perhaps reflecting the social nature of much work.

The effects of Culture on Education and Occupation are largely independent of ability and high school grades. Since Culture measures the extent to which the student identifies with a group that values education, we may infer that it influences education because it reflects the student's values.

The Leadership composite is the student's perception of his peers' judgment of him. It may reflect his ability to accomplish concrete goals in a high school peer-group context. Whatever personality factors go into this perceived ability must be somewhat stable, since Leadership exercises an appreciable effect on earnings 12 years later, even when background and intervening factors are controlled. I therefore suspect that the measure captures social skills that are useful in a wide variety of circumstances.

2. Indirect Measures of Personality

If eleventh graders have stable personality characteristics that affect later success, the students' life styles and attitudes should reflect these characteristics. From among the questions on Talent's high school questionnaire, I selected 60 that seemed best to describe the way the student interacted with his environment. These questions cover six broad areas: 1) study habits and attitudes, 2) participation in group activities, 3) participation in other activities, 4) attitudes, 5) ability-related characteristics, and 6) physical characteristics.

It was not possible to examine effects of student responses to all 60 questions at once, since very few students answered every question. To increase reliability and to aid in interpretation, I therefore combined related questions into composites. My method was to some degree *ad hoc*, since I looked at questions' correlations with later success before deciding to combine them. Once I decided to combine a set of questions, however, I either used the first principal component (which depends on the correlation among questions being combined) or an *a priori* weighting scheme. To test whether a composite captured the effects of all the individual questions, I first regressed Education, Occupation, Hourly Earnings and Ln Hourly Earnings on the composite, controlling measured family background characteristics. I considered the composite to be adequate if none of the component questions added appreciably to the proportion of variance explained by the composite alone.[6/] No composite adequately represented certain sets of questions. I kept these separate in my analyses.

Study Habits is the first principal component of 14 questions which measure the extent to which a student accepts his teachers' norms regarding academic work, or at least says he accepts them. (Table 5.9 presents questions and codings.) Students receive a high score if they pay attention in class, keep up to date on their assignments, do more work in a course than is required, or spend many hours on homework. If such behavior persists when students take a job, and if employers value it, high scorers should earn more than low scorers (Bowles and Gintis, 1976).

Best Work is based on a 15th question about study habits, namely whether the student says he often does assignments "so quickly that I don't do my best work." Teachers presumably prefer students who never do assignments too quickly. Although this question correlated positively with Study Habits, its effects on outcomes were different.[7/]

[6/] I also performed this test with test score controlled. In addition, I checked for non-linear effects after controlling the composite. Thus, if any question in a composite had a substantively important effect - linear or nonlinear - not reflected in the composite, I rejected the composite.

[7/] Including Best Work in the Study Habits composite led to a serious underestimate of the effects of these questions. This suggests that even when different questions appear to measure the same personal trait, they may not do so. It also underlines the danger of using a priori composites without considering whether the items all measure the same underlying traits. Factor analysis alone does not suffice to answer this question. This caveat applies with special force to my earlier analyses of self-assessed traits, where I was not able to test the adequacy of Talent's a priori composites.

Table 5.9 Study Habits and Best Work: Codings for Talent Questions

	Code
For the following statements indicate how often each one applies to you. Please answer the questions sincerely. Your answers will not affect your grades in any way. Mark one of the following choices for each statement. A. Almost always, B. Most of the time, C. About half the time, C. Not very often, A. Almost never.	
Study Habits (principal component 14 questions)[c]	
I do a little more than the course requires.	a
I make sure that I understand what I am to do before I start an assignment	a
Lack of interest in my school work makes it difficult for me to keep my attention on what I am doing.	b
Failure to pay attention in class has caused my marks to be lowered.	b
I consider a very difficult assignment a challenge to my abilities.	a
I have missed assignments or other important things that the teacher has said, because I was not paying attention.	b
My teachers have criticized me for turning in a sloppy assignment.	b
Unless I really like a course, I do only enough to get by.	b
In class I can't seem to keep my mind on what the teacher is saying.	b
I get behind in my school assignments.	b
I feel that I am taking courses that will not help me much in an occupation after I leave school.	b
I don't seem to be able to concentrate on what I read. My mind wanders and many things distract me.	b
I keep up to date on assignments by doing my work every day.	a
On the average, how many hours do you study each week? Include study periods in school as well as studying done at home.	
A. None	0
B. About 1-4 hours per week	3
C. About 5-9 hours per week	7
D. About 10-14 hours per week	12
E. About 15-19 hours per week	17
F. About 20 or more hours per week	22
Best Work	
I do my assignments so quickly that I don't do my best work.	b

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[a] A=5, B=4, C=3, D=2, E=1.

[b] A=1, B=2, C=3, D=4, E=5.

[c] The principal component explained 30.8 percent of the variance in the questions.

Affiliations (+) and Affiliations (-) measure student participation in group activities (see Table 5.10). Affiliations (+) is the principal component of the 7 group participation questions that had positive effects on outcomes. These included membership in church groups, social clubs, and clubs dealing with school subject matter. Affiliations (-) is the principal component of those membership questions with negative effects on outcomes, including membership in farm youth groups, political clubs, military or drill units, and hobby clubs. I have no convincing explanation why the first set of group memberships aids later achievement while second set depresses it. The two types of group membership are not negatively correlated with one another. Instead, the correlation is positive, suggesting that individuals who join one group are likely to join others as well. But whatever "joiners" share is either not stable or has little effect on later success.

Leadership Roles is a composite constructed by Talent researchers on the basis of leadership positions the student reported he held in various student groups (see Table 5.11). Given the effect of the Leadership self-assessment discussed in the previous section, I expected it to have a positive effect on later achievement, especially on earnings.

Dating

Talent asked four questions about students' dating experience and involvement in social recreation. I was unable to devise a composite that captured these variables' effects, so I retained the individual questions in later analyses. The questions ascertain the age the student started dating, the number of dates he had per week, how

Table 5.11 Codings of Talent Questions on Leadership Roles

Leadership Roles (Constructed by Talent)

	Response Option	Talent Weight
How many times have you been president of a class, a club, or other organization (other than athletic) in the last 3 years? How many times in the last 3 years have you been captain of an athletic team?	None	0
	Once	12
	Twice	14
	Three Times	16
	Four Times	18
	Five or More Times	20
	Omit	0
	MD	
How many times have you been an officer or committee chairman (other than president) of a class, a club, or other organization (other than athletic) in the last 3 years?	None	0
	Once	6
	Twice	7
	Three Times	8
	Four Times	9
	Five or More Times	10
	Omit	0
	MD	

The range of possible scores is from 0 to 50.

Table 5.10 Codings for Talent Questions on Student Group Participation

Items	Code
How active have you been in any one or more of the following organizations? Mark your answers as follows: A. Extremely active, B. Very active, C. Fairly active, D. A member, but not very active, E. A member but rarely active, F. Not a member of any of these organizations.	A=5, B=4, C=3, D=2, E=1, F=0
Affiliations (+) (principal component 7 questions)[a/]	
- School newspaper, magazine or annual	
- School subject matter clubs, such as science, mathematics, language or history clubs	
- Debating, dramatics, or musical clubs or organizations	
- Church, religious, or charitable organization, such as Catholic Youth of America, B'nai B'rith Youth Organization, Protestant youth group; organized non-school youth groups such as YMCA, YWCA, Hi-Y, Boy's Club, etc.	
- Informal neighborhood group	
- Social clubs, fraternities, or sororities	
- How many athletic teams have you been a member of in the last 3 years? Count intra-mural, church, school, and other teams.	
A. None, B. One, C. Two, D. Three, E. Four, F. Five, G. Six, H. Seven, I. Eight, J. Nine, K. Ten, L. Eleven or more	A=1, B=2, C=3, D=4, F=5, G=6, H=7, I=8, J=9, K=10, L=11
Affiliations (-) (principal component 4 questions)[b/]	
- Political club, such as Young Democrats or Republicans	
- Military or drill units	
- Hobby clubs, such as photography, model building, hot rod, electronics, woodworking, crafts, etc.	
- Farm youth groups, such as 4-H club, Future Farmers of America, etc.	

[a/] First principal component explained 31.0 percent of the variance in the 7 questions.

[b/] First principal component explained 39.1 percent of the variance in the 4 questions.

often he had gone steady, and how many times per week he went out for recreation (see Table 5.12). I coded each variable in its natural metric (i.e., age or number of times). For the three dating variables I also included a dummy to identify individuals with no dating experience whatever. Following Coleman (1961), we might expect social activity to imply involvement in an adolescent subculture that discouraged intellectualism. If so, it should be negatively associated with educational attainment. Socially involved individuals may also have less taste for school work. Alternatively, individuals who are less interested in school may become involved in social activities. However, later achievements are likely to require social skills, so socially involved students may not suffer the same disadvantage in terms of earnings as in terms of educational attainment.

I measured student employment experience by the number of hours per week the student worked during the school year (see Table 5.12). I also considered three other measures of employment experience, the age the student first began working, the number of summers he had worked, and the percentage of his spending money coming from a job. However, Hours Worked had a larger effect on all outcomes than any of these questions, and after I controlled Hours Worked, none of the others had a statistically significant effect, so I dropped them in later analyses.

Although we might expect individuals who had held down jobs to be more oriented towards achievement, especially in nonacademic pursuits, high school employment is actually negatively associated with educational and occupational attainment, and has no effect on

Table 5.12 Codings for Talent Questions Relating to Various Student Activities

Social Activities	Code	Code for Dummy
Age on First Date How old were you when you first went out on a date? A. I have never been on a date. B. 12 or younger C. 13 or 14 D. 15 E. 16 F. 17 or older	A=19, B=10, C=13.5, D=15, E=16, F=17	A=1, others=0
Dates per Week On the average, how many dates do you have in a week? A. I never have dates. B. About 1 C. About 2 D. About 3 E. About 4 or 5 F. About 6 or 7	A=0, B=1, C=2, D=3, E=4.5, F=6.5	A=1, others=0
Times Gone Steady How many times have you gone "steady" in the past three years? A.None B.Once C.Twice D.Three times E.Four times F.Fives times or more	A=0, B=1, C=2, D=3, E=4, F=6	A=1, others=0
Time Out per Week On the average, how many evenings a week during the school year do you usually go out for fun and recreation? A. Less than one B. One C. Two D. Three E. Four or five F. Six or seven	A=.3, B=1, C=2, D=3, E=4.5, F=6.5	
Hours per Week Worked		
During the school year, about how many hours a week do you work for pay? Do not include chores done around your own home. A. None B. About 1-15 hours C. About 6-10 hours D. About 11-15 hours E. About 16-20 hours F. About 21 hours or more	A=0, B=3, C=8, D=13, E=18, F=22	
Intellectual Reading (principal component 4 questions)[a/]		
How many books have you read (not including those required for school) in the past 12 months? Don't count magazines or comic books. A. None B. 1 to 5 C. 6 to 10 D. 11 to 15 E. 15 to 20 F. 21 or more	A=0, B=3, C=8, D=13, E=18, F=21	
How many books or magazines have you read in each of the following groups (not including those required for school) in the past 12 months? Mark your answers as follows: A. None B. 1 C. 2 D. 3 E. 4 F. 5 or more - Science - non-fiction - Plays, poetry, essays, literary criticism or classics - Politics, world affairs, biography, autobiography, historical novels	A=0, B=1, C=2, D=3, E=4, F=6	
Science Fiction Reading		
Science Fiction books or magazines (not comic books)		

[a/] Principal component explained 56.2 percent of the variance in the 4 questions.

Table 5.12 continued

Hobbies (Constructed by Project Talent)

How often have you done any one or more of the following in the past 3 years? Include extra-curricular activities at school, but do not include things done for school assignments. In each groups of activities, answer for one or more in the group. Mark your answers as follows: A. Very often, B. Often, C. Occasionally, D. Rarely, E. Never.

Response Option	Talent Weight
A	6
B	4
C	2
D	1
E	0
Omit	0
MD	-

- Drawing, painting, sculpting, or decorating
- Acting, singing or dancing for a public performance
- Collecting stamps, coins, rocks, insects, etc.
- Building model airplanes, ships, trains, cars, etc.
- Working with photographic equipment (do not include taking occasional snapshots)
- Making jewelry, pottery or leatherwork
- Making or repairing electrical or electronic equipment
- Cabinet making or woodworking
- Metal working
- Mechanical or auto repair
- Raising or caring for animals or pets
- Sewing, knitting, crocheting or embroidering
- Cooking
- Gardening, raising flowers or raising vegetables

How often have you done any one or more of the following in the past 3 years? Mark your answers as follows: A. Very often, B. Often, C. Occasionally, D. Rarely, E. Only Once, F. Never.

- Attending concerts, lectures, plays (not motion pictures), ballet, visiting art galleries or museums

Response Option	Talent Weight
A	6
B	5
C	3
D	2
E	1
F	0
Omit	0
MD	-

Possible scores range from 0 to 90

Table 5.12 continued

Participation in Sports (Constructed by Project Talent)

How often have you done any one or more of the following in the past 3 years? In each group of activities, answer for one or more in the group. Mark your answers as follows: A. Very often, B. Often, C. Occasionally, D. Rarely, E. Never.

- Playing baseball, football or basketball

Response Option	Talent Weight
A	6
B	4
C	2
D	1
E	0
Omit	0
MD	-

How often have you done any one or more of the following in the past 3 years? Mark your answers as follows: A. very often, B. Often, C. Occasionally, D. Rarely, E. Only once, F. Never

- Play golf or tennis, swimming
- Play hockey, lacrosse, or handball; boxing, wrestling, track, field events
- Go bicycling, ice skating, skiing, canoeing, horseback riding

Response Option	Talent Weight
A	6
B	5
C	3
D	2
E	1
F	0
Omit	0
MD	-

Possible scores range from 0 to 24

Cultural Events

How often have you done any one or more of the following in the past 3 years? Mark your answers as follows: A. Very often, B. Often, C. Occasionally, D. Rarely, E. Only once, F. Never

A=0, B=1, C=2, D=3, E=4, F=5

- Attending concerts, lectures, plays (not motion pictures), ballet, visiting art galleries or museums earnings, even after family background is controlled. The fact that other employment experiences have no impact after controlling for work during the school year suggests that those who have held jobs do not differ from others in any consistent way. Hours worked during the school year may therefore lower educational attainment and occupational status either because students who are less concerned or interested in academic matters spend more time working during the school year, or because the student's job leaves less time for school work. Hours Worked may also reflect aspects of background not captured by my controls. In any case, these analyses do not suggest that employment experiences in high school measure any motivational characteristic that influences later success.

Intellectual Reading is the principal component of four questions on the nonrequired reading done by students. These include a measure of the total number of books read, as well as measures of reading in science, literature, politics and history (see Table 5.12). Although I expected intellectualism to be associated with greater educational attainment, its effects on Occupation and Hourly Earnings are not so easy to predict, *a priori*. Since intellectuals prefer jobs requiring cognitive skills, and since such jobs generally have higher status and pay more, intellectualism may be positively associated with status and earnings. However, the preferences of such individuals may cause them to trade off money or even status for intellectual challenge. Earnings are probably easier to trade away than is status, since a certain amount of status is linked with intellectual pursuits per se. If so, intellectualism is likely to be less helpful for earnings than status.

Science Fiction Reading is ascertained from a single question. Although positively associated with other nonrequired reading, science fiction reading is negatively associated with educational attainment. It is probably best thought of as interest that diverts attention from academic pursuits.

Student interest in high culture is ascertained from a question that asks how often the student attended concerts, lectures, plays or other similar events. I used composites constructed by Talent researchers to measure student interests in hobbies and involvement in various sports (see Table 5.12).[8/] Preliminary analyses indicated that although participation in sports was positively associated with later outcomes after controlling family background, it had no statistically significant effect. I therefore omitted it from later analyses.

The five variables in Table 5.13 measure students' attitudes rather than their behavior.

Importance of Insurance is the student's response to a single question on the importance of life insurance, coded on a equal interval five category scale (see Table 5.13). Three other questions relating to financial security (expected life insurance in terms of future salary; expected savings in terms of salary; expected investments in securities in terms of salary) had smaller effects than Importance of Insurance. None of them had a significant effect after controlling Importance of Insurance.

Importance of Education is the respondent's view of whether it is necessary to have a college education to be a leader in the community.

8/ I used these two composites without testing whether they adequately reflected the effects of their component questions. Effects of individual questions may therefore be hidden.

Table 5.13 Codings for Talent Questions on Student Attitudes

	Code
Insurance Important	
For a man who has a wife and children, having a life insurance policy is: A. extremely important B. very important C. important D. neither important nor unimportant E. unimportant F. not at all important	A=6, B=5, C=4, D=3, E=2, F=1, no answer=4 a/
Education Necessary	
For each of the following statements indicate how much you agree or disagree. Mark one of the following choices for each statement: A. Agree strongly B. Agree C. Neither agree nor disagree D. Disagree E. Disagree strongly	
It is not necessary to have a college education to be a leader in the community.	A=1, B=2, C=3, D=4, E=5, No answer=6 a/
Work Orientation	
Imagine that you have been working for an employer for several years. How important do you think each of the following conditions would be in influencing you to quit to go to work for another employer? Mark your answers as follows: A. Extremely important B. Very important C. Important. D. Neither important nor unimportant E. Unimportant F. Not at all important	
- Materialistic. If I could get better pay at another place.	A=6, B=5, C=4, D=3, E=2, F=1, No answer=2 a/
- Interest. If the work was not interesting enough.	As above
- Advancement. If I do not receive expected promotions or salary increases.	As above

a/ Nonresponse for these questions ranged from 28 to 46 percent. Since the absence of specific plans or ideas about the future is itself a personal characteristic, it did not make sense to omit individuals who did not respond. Instead, I ran regressions of Education, Occupation, Hourly Earnings and Ln Hourly Earnings on each question, assigning nonrespondents a valid value and including a dummy for non-response. For every question but one, the coefficient of the dummy indicated that in terms of education, occupational status, and earnings, non-respondents were like some specific group of respondents. For example, 38 percent did not answer the question on the importance of life insurance. They were similar on all outcomes to men who said life insurance was "important." I therefore coded the two groups in the same way. This was also possible for other questions.

I also considered a question that asked students whether "girls should go to college only if they plan to use their education on a job," but found it had no effect after controlling importance of Education.

Materialistic Orientation tries to measure the extent to which the student views adult work in terms of monetary rewards. Interest Orientation tries to measure the importance of intrinsic rewards to work. Advancement Orientation tries to measure the value of continued promotions and raises. Each question asks how likely the student would be to quit a job if it did not meet the particular standard. Correlations between these items range from 0.6 to 0.7, perhaps reflecting the fact that for all questions a positive answer implies taking purposive action.

Perception of Ability is the principal component of six questions that ask the student to judge his own academic skills, including his reading and writing ability and his studying skills (Table 5.14). This measure naturally reflects actual ability. It is only of interest insofar as it predicts success after controlling for measured cognitive ability.

Grades. Students' grades in history and social studies courses had larger effects on all outcomes than did grades in English, mathematics, foreign language, or science courses. History and social studies grades also had a greater effect on outcomes than did

Table 5.14 Codings for Talent Questions Ascertaining Traits Affected by Cognitive Ability

	Code
For the following statements indicate how often each applied to you. Please answer the questions sincerely. Your answers will not affect your grades in any way. Mark the one of the following choices for each statement. A. Almost always B. Most of the time. C. About half the time. D. Not very often. E. Almost never.	
Perception of Ability (principal component questions)[c/]	
- I seem to accomplish very little compared to the amount of time I spend studying.	b
- I enjoy writing reports or compositions.	a
- I have difficulty with the mechanics of English composition.	b
- My grades on written examinations or reports have been lowered because of careless errors in spelling, grammar or punctuation.	b
- When studying for a test I am able to pick out important points to learn.	a
- I have trouble remembering what I read.	b
Grades . The following questions ask you to report your grades in courses you have taken in the ninth grade or later. Please consider only semester grades. If you have not taken any courses in the topic, skip the item. In these questions choose the one answer that best describes your grades. Mark your answers as follows: A. All A's or equivalent B. Mostly A's or equivalent C. Mostly A's and B's or equivalent D. Mostly B's and C's or equivalent E. Mostly C's and D's or equivalent F. Mostly D's or below or equivalent.	A=6, B=5, C=4 D=3, E=2, F=1
If your school does not use letter gardes, please use the following equivalents: For a grade of A: Excellent; 90-100, For a grade of B; good; 80-90, For a grade of C: Average; 70-79, For a grade of D; Fair; 60-69, For a grade below D; Failing; 59 or lower.	
- My grades in mathematics have been:	
- My grades in science courses have been:	
- My grades in foreign languages have been:	
- My grades in history and social studies courses have been:	
- My grades in all courses starting with ninth grade have been:	

a/ A=5, B=4, C=3, D=2, E=1

b/ A=1, B=2, C=3, D=4, E=5

c/ The principal component explains 34.3 percent of the variance in the 6 questions.

average grades in all four academic subject areas.[9]

I also examined the student's report of his height, and a measure of obesity based on weight and height. Neither measure had a statistically significant effect on any outcome after family background was controlled. I therefore omitted these items from later analyses.[10]

Determinants of Indirect Measures of Personality

Table 5.15 provides regressions of the noncognitive measures on family background characteristics. Measured background explains 8 percent of the variance in Affiliations (-), 6 percent of the variance in attendance at cultural events, and less than 5 percent of

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[9] The coding I used for grades differs from the standard five-step scale (i.e., A=4 to F=0). I coded student responses into the standard scale, but found that correlations with dependent variables fell slightly.

Differences between subjects are not due to sampling error. We might expect that these differences would be due to the relative importance of cognitive and noncognitive factors for grades in different subjects. Thus, grades in those subjects which were less well predicted by ability would have stronger effects because they would be more likely to reflect noncognitive factors. This does not appear to be the case. Whereas we expect grades in those courses that have strongest effects to have lower correlations with Test Score, we find no such pattern.

[10] My analyses do not disprove Deck's (1968) contention that height influences salary. In my analyses, the coefficient of height had the expected positive sign in predicting all outcomes after controlling background. It is possible that if it was measured later, at the time of first job interview as was Deck's measure, or at some other more appropriate time, it could have a moderate effect on economic success.

Similarly, although again effects were not statistically significant, obesity had an identical curvilinear effect on all dependent variables after controlling family background. Students at or slightly below the average on the dimension of weight had greater achievements than did underweight or obese students.

Table 5.15: Regressions of Indirect Measures of Personality on Family Background Characteristics: Talent Males.

Standardized Coefficients with Background Measures
but No Test Score

	White	Father's Education	Father's Occupation	Father Absent	Siblings	$\bar{R}^{2}$ a/	$\bar{R}$ b/	Test Score	$\bar{R}$ c/
Academic Response									
Study Habits	[-.064]	.099	.084	[-.062]	[-.062]	.032	.045	.335	.131
Best Work (#75)	[-.006]	[.033]	[.008]	[.003]	[.027]	.000	.000	.094	.002
Group Activities									
Affiliations (+)	-.185	.116	.089	-.068	[-.029]	.061	.078	[-.025]	.078
Affiliations (-)	[-.137]	[-.002]	[-.063]	[.022]	[.060]	.028	.027	-.206	.089
Leadership Roles	-.116	.073	.088	[.022]	[-.050]	.029	.032	[.005]	.031
Social Activities									
Age 1st Date (#51)	[-.048]	-.090	[.016]	[.041]	.109	.022	.029	[.031]	.029
Times Gone Steady (#54)	-.116	[.041]	[-.050]	[.064]	[-.006]	.017	.018	-.195	.047
Times Out per Week (#55)	[-.035]	[-.056]	[-.049]	[.114]	[.018]	.019	.025	-.127	.037
Hours Worked per Week (#37)	[.031]	-.080	[-.013]	[.060]	[.030]	.008	.018	-.132	.026
Reading									
Intellectual Reading	[-.045]	.126	[.027]	[.031]	[.045]	.018	.028	.103	.036
Sci. Fic. Reading (#58)	[-.060]	[.033]	[.006]	[-.008]	[.020]	.000	.000	[.038]	.000
Cultural Events	-.071	.076	.179	[-.015]	[-.001]	.049	.060	.129	.072
Hobbies	-.107	[.009]	[.016]	[-.009]	[.029]	.008	.011	-.175	.033
Attitudes									
Importance of Insurance (#365)	.086	[-.032]	-.082	-.084	-.102	.033	.038	.208	.070
Education Necessary (#336)	.073	[-.001]	.109	-.081	-.075	.032	.029	.161	.048
Work Orientation: Materialistic (#356)	[.059]	[-.025]	[.049]	[-.062]	[-.052]	.009	.011	.100	.018
Interest (#357)	[.055]	[-.026]	.107	-.097	-.066	.027	.029	.209	.062
Advancement (#361)	[.050]	[-.019]	[.061]	-.076	-.053	.012	.014	.138	.027
History, S.S. Grades	-.085	.112	.077	[-.012]	-.069	.034	.046	.334	.131

Notes:

Sample 932 individuals with complete data o background controls, Test Score, High School Curriculum, Years of Education, as well as indirect measures of personality.

a/ Proportion of variance explained with only 5 linear family background characteristics controlled.

b/ Proportion of variances explained when, in addition to the 5 characteristics, Talent's Socioeconomic Index and 5 selected interactions (standard background controls) are entered.

c/ Proportion of variance explained when the Test Score is entered in addition to the 11 family background controls.

the variance in the other noncognitive measures. Students whose fathers have more education or higher status jobs have more "favorable" traits. Individuals from large families and broken homes do not differ consistently from others, except on some of the attitude measures.

The last two columns of Table 5.15 show the effects of Test Score on each personality trait with background controlled. These equations are, of course, only meaningful if test scores affect personality but not vice versa. Except for Grades and Study Habits, the coefficients are 0.21 or less. Test Score usually has more impact than background, but this is not always true.

Indirect Measures of Personality: Total Effects

Effects on Education

Table 5.16 presents effects for the indirect measures of noncognitive traits after background characteristics are controlled. Coefficients are shown when measures enter separately (Column 2), when they are entered with other similar measures (Column 3), and when they all enter together (Columns 4 and 5). For example, the Study Habits composite has a standardized coefficient of 0.279 with only family background controlled, but when we control all other indirect measures of personality, its coefficient declines to 0.187. Column 3 indicates that when all three measures of Work Orientation are considered together, those who think of a job only in terms of money obtain less schooling, while those who are concerned with the intrinsic characteristics of a job obtain more schooling.

Table 5.16: Standardized Regressions of Education on Indirect Measures of Personality with Background Characteristics, and Test Score Controlled: Talent Males

Controls	(1) None r	(2) Background[a]	(3) Background and Personality Measures in Group[b]	Combined Beta	(4) Background and All Personality Measures	Combined Beta	(5) Background and Significant Personality Measures
Academic Response							
Study Habits:	.376	.279			.187		.192
Best Work	.091	.075			[.000]		*
Group Activities							
Affiliations (+)	.246	.149	.474	.225	.118	.147	.125
Affiliations (-)	-.131	-.089	-.442		-.088		-.086
Leadership Roles	.170	.095	.009		.059		.056
Social Activities[d]							
Age 1st Date	-.035	.046	.153	.248	.125	.180	-.154
Never Dated	.082	.120					.193
Times Gone Steady	-.163	-.152	-.131		.065		-.068
Times Out per Week	-.195	-.154	-.138		-.109		-.106
Hours per Week Job	-.215	-.149			-.091		-.093
Reading							
Intellectual Reading	.178	.119			[.054]		*
Sci. Fic. Reading	.010	[.001]			[.047]		*
Cultural Events	.299	.195	.241	.232	.125	.138	.135
Hobbies	-.052	-.049	-.129		-.114		-.118
Attitudes							
Importance of Insurance	.201	.136	.104	.175	[.064]	.119	.090
Education Necessary	.160	.127	[.053]		[.028]		*
Work Orientation:							
Materialistic	.087	[.045]	-.122		-.097		*
Interest	.184	.124	.114		[.059]		*
Advancement	.147	.098	[.025]		[.058]		*
$\bar{R}^2$					.422		.417

Table 5.16 (continued)

Controls	(6) Background and Test Score	(7) Background, Test Score and Personality Measures in Group	(7) Combined Beta	(8) Background, Test Score and All Personality Measures	(8) Combined Beta	(9) Background, Test Score and Significant Personality Measures
Academic Response				.428		
Study Habits:	.161			.109		.118
Best Work	.044			[.004]		
Group Activities						
Affiliations (+)	.146	.151		.111		.115
Affiliations (-)	.001	-.065	.165	[-.038]	.134	
Leadership Roles	.092	[.048]		.055		
Social Activities						
Age 1st Date	.030	.092		.089		-.096
Never Dated	.071		.155		.131	.133
Times Gone Steady	.077	-.062		[-.034]		
Times Out per Week	-.107	-.104		- .088		.090
Hours per Week Job	-.108			-.084		- .089
Reading						
Intellectual Reading	.067			[.033]		
Sci. Fic. Reading	[-.019]			-.053		
Cultural Events	.142	.161	.152	.100	.102	.112
Hobbies	[.006]	[-.052]		-.074		-.083
Attitudes						
Importance of Insurance	.067	[.061]		[.048]		.058
Education necessary	[.034]	[.018]		[.010]		
Work Orientation:	[.010]	[-.071]	.086	[.064]	.076	
Materialistic						
Interest	[.045]	[.030]		[.010]		
Advancement	[.051]	[.037]		[.058]		
$\bar{R}^2$				.493		.490
History S.S. Grades	.194			.128		.134
Perception of Ability	.105			[-.011]		
$\bar{R}^2$				.504		.503

Notes:

Sample 932 individuals with complete data on background controls, Test Score, High School Curriculum, Years of Education, as well as the 21 indirect measures of personality listed in the table.

* Entered if significant.

a/ Standardized coefficients for each measure when it is entered alone after controls.

b/ Standardized coefficients for similar measures entered together after controls. Combined Beta is the total effect of these measures entered together, and may be interpreted as their influence before controlling other noncognitive characteristics.

c/ All noncognitive measures entered. Combined Beta is the total effect for similar measures together, and may be interpreted as their influence after controlling other noncognitive characteristics.

d/ Omitted from this table were Dates Per Week, and dummy variables for not dating and having never gone steady. These had no effect on Education after controlling other social activities variables.

Standardized coefficients for variables which derive from the same questions are not meaningful when entered together. For example, age at first date and the dummy for having never been on a date cannot be separated, and therefore do not have substantively meaningful standardized coefficients when they are entered together. Even coefficients for variables based on different questions must be interpreted with caution unless the questions measure traits that are conceptually distinct. Thus while membership in one set of groups is separable from membership in another, a person's various affiliations are probably also interrelated. It may not be possible to alter an individual's membership in one group without altering his social contacts, and thus his membership in other groups. Thus, we should not necessarily attach causal importance to a coefficient for one Affiliations measure while the other is controlled.

In order to provide interpretable coefficients, I have combined coefficients for variables which refer to the same question. In addition, I also present combined coefficients for groups of variables. Thus, the three questions which deal with student group affiliations have a combined effect of 0.147 in regression 4. This is the effect of having helpful characteristics measured by all three variables.

These coefficients are, of course, only meaningful if they measure distinct traits. I have not tried to prove that they do. Nonetheless, I will proceed as if each label really identified a conceptually distinct trait. Thus, column 3 implies that having the "right" response to academic demands, having an anemic social life, and being affiliated with certa[illegible]ds of groups are all about equally helpful in pushing the high school student towards additional education.

Column 4 shows that all these traits have effects that are somewhat but not completely independent of one another. Similarly, Column 4 shows that holding down a job during the school year depresses educational attainment less with other noncognitive traits controlled. Apparently, the working student suffers in part because his other measured noncognitive traits put him at a disadvantage.

Conspicuous by their small and only occasionally significant effects are the attitude questions. If students who have appropriate attitudes are much more successful than others, our measures must not measure the relevant attitudes. Nor does extracurricular reading have a statistically significant effect after other measures are controlled.

No single noncognitive characteristic appears central in determining how much education an individual will obtain. Instead, each of several measures has a small but distinct effect on educational attainment. **Correlations between these measures are sometimes positive and sometimes negative.** Background alone explains 24.6 percent of the variance in education. Adding all the measures of noncognitive traits together (excluding grades and perceptions of ability) increases $\bar{R}^2$ to 0.422.

Columns 6 to 9 present effects of these same noncognitive measures under the assumption that they depend on academic ability and that one should therefore assess their effects with ability controlled. Although their impact is clearly lessened, the measures increase $\bar{R}^2$ from 0.420 to 0.493, and the increment remains highly significant.

I have also presented the effects of History and Social Studies Grades and Perception of Ability, since these measures depend on cognitive ability. Column 8 indicates that once other noncognitive characteristics are controlled, Perception of Ability does not affect educational attainment, while grades do.[11/]

Effects on Occupation

The regression of Occupation on indirect measures of noncognitive traits, controlling background characteristics (Table 5.17), yields few surprises. Combined coefficients for three classes of variables, those relating to study habits, membership and position in groups, and social activities are similar (0.165, 0.160, and 0.188). Effects of extracurricular reading are small and not statistically significant, while attitude measures have minimal effects. As was the case with education, no single noncognitive factor dominates the others.

Controlling academic ability reduces coefficients by as much as a third (see Columns 6 to 9). Variables relating to student social activities become relatively more important, however. Controlling for education as well as ability (Column 10) further reduces the coefficients, leaving only measures of leadership and social activities statistically significant.

[11/] Note that in Columns 8 and 9 the coefficients for Grades and Perception of Ability are estimated with all noncognitive measures, background and cognitive ability controlled. The value of $\overline{R}^2$ at the very bottom of the column is for this full regression. However, coefficients for the other noncognitive measures refer to regressions that do not control Grades and Perception of Ability. The first $\overline{R}^2$ shown refers to this regression.

Table 5.17 Standardized Regressions of Occupation on Indirect Measures of Personality with Background Characteristics and Test Score Controlled: Talent Males.

Controls	(1) None	(2) Background	(3) Background and Personality Measures in Group	Combined Beta	(4) Background and All Personality Measures	Combined Beta	(5) Background and Significant Personality Measures
	r						
Academic Response							
Study Habits	.302	.236			.172	.165	.175
Best Work	.000	-.008			-.073		-.074
Group Activities							
Affiliations (+)	.196	.130	.143		.075		.079
Affiliations (-)	-.127	-.085	-.156	.223	-.100	.160	-.099
Leadership Roles	.186	.137	.114		.111		.108
Social Activities							
Never Dated	.091	.117	.139		.133		.137
Doesn't Date	-.010	[.014]	-.092	.210	-.097	.188	-.098
Time Out per Week	-.208	-.172	-.171		-.147		-.142
Hours per Week Job	-,160	-.119			-.079		.079
Reading							
Intellectual Reading	.134	.090			[.028]		*
Sci. Fic. reading	.010	[.008]			[-.021]		*
Cultural Events	.251	.178	.212	.201	.121	.124	.127
Hobbies	-.030	-.022	-.093		-.088		-.089
Attitudes							
Importance of Insur.	.160	.110	.096		[.056]		.070
Educ. Necessary	.118	[.065]	[.035]		[.027]		*
Work Orient: Material	.056	[.031]	[-.086]	.130	[-.071]	.095	*
Interest	.119	.075	[.041]		[.002]		*
Advancement	.113	.080	[.042]		[.070]		*
$\bar{R}^2$					.267		.267

Table 5.17 (continued)

Controls	(6) Background and Test Score	(7) Background, Test Score and Personality Measures in Group	(7) Combined Beta	(8) Background, Test Score and All Personality Measures	(8) Combined Beta	(9) Background, Test Score and Significant Personality Measures	(10) Background, Test Scores Education and Significant Personality Measures
Academic Response							
Study Habits	.143			.110	.110	.113	*
Best Work	[-.016]			-.067		-.067	*
Group Activities							
Affiliations	.134	.113	.175	[.070]	.143	*	*
Affiliations	[.010]	[-.054]		[-.044]		*	*
Leadership Roles	.138	.106		.106		.112	.090
Social Activities							
Never Dated	.077	.111	.173	.114	.165	.114	.073
Doesn't Date	[-.023]	-.107		-.097		-.103	-.090
Time Out per Week	-.135	-.142		-.130		-.124	-.093
Hours per Week Job	-.081			-.070		-.075	*
Reading							
Intellectual Reading	[.060]			[.022]		*	*
Sci. Fic. Reading	[.000]			[-.026]		*	*
Cultural Events	.137	.145	.139	.095	.092	.091	*
Hobbies	[.029]	-.023		[-.055]		*	*
Attitudes							
Importance of Insur.	[.044]	[.052]	.063	[.037]		*	*
Educ. Necessary	[.016]	[.011]		[.012]		*	*
Work Orient: Materi	[-.004]	[-.046]		[-.046]		*	*
Interest	[.005]	[-.023]		[-.037]		*	*
Advancement	[.034]	[.045]		[.063]		*	*
$\bar{R}^2$				.313		.310	.427
History, S.S. Grades	.160			.096			
Perception of Ability	.082			[-.020]			
				.318			

Notes:

Sample 836 individuals with complete data on background controls, Test Score, High School Curriculum, Years of Education, Occupation as well as the 21 indirect measures of personality listed in the table.

a/ Omitted from this table were Age of 1st Date, Dates per Week and Times Gone Steady and the dummy for having never gone steady. These had no effect on Occupation after controlling other social activities variables.

* Entered if significant.

Background explains 12.7 percent of the variance in occupational status. The noncognitive measures raise this to 26.7 percent. If we enter the noncognitive measures after academic ability (Column 8) $\bar{R}^2$ rises from 0.241 to 0.318.

Effects on Earnings

Table 5.18 presents regressions of Hourly Earnings at 28 on those noncognitive measures that had significant effects in preliminary analyses.[12]

The noncognitive traits have small effects. Considered separately (Column 2) Leadership Roles has the largest single effect (coefficient 0.114).

Although Study Habits has a positive effect on earnings, the "Best Work" measure has a negative effect, implying that those who say they often do assignments too quickly to do their best work make more money. We might take this as an indication that the pragmatist has an advantage over the perfectionist after he leaves school. Alternatively, individuals who say they never work too quickly to do their best work may have lower personal standards, so the negative sign may indicate that those who set high standards for themselves obtain more earnings, even when they do not obtain more schooling.

Never having gone steady has a negative coefficient in the regression, indicating that students who had gone steady are more

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12/ The sample I used to calculate regressions in Table 5.18 was smaller than samples used for preliminary analyses. Separate effects (Column 2) are therefore not always statistically significant, although they were in the larger samples.

Table 5.18: Regressions of Hourly Earnings on Indirect Measures of Personality with Background Characteristics and Test Score Controlled: Talent Males

	Standardized Coefficients				
	(1)	(2)	(3)	(4)	(5)
Controls	None	Background	Background and Attitudes	Background and All Personality Measures	Background and Significant Personality Measures
Academic Response					
Study Habits	.102	[.066]		.100	.105
Best Work	-.054	[-.060]		-.086	.087
Leadership Roles	.134	.114		.096	.094
Never Gone Steady	-.088	-.088		-.081	-.081
Intellectual Reading	-.041	-.065		-.095	-.093
Attitudes					
Importance of Ins.	.079	[.060]	[.005]	[.002]	
Educ. Necessary	.092	.075	[.049]	[.052]	.074
Work Orientation:					
Material	.071	[.063]	[-.010]	[-.013]	*
Interest	.099	.089	[.072]	[.063]	*
Advancement	.073	[.065]	[.006]	[-.000]	*
$\bar{R}^2$				.054	.056

Table 5.18 (continued)

Controls	(6) Background and Test Score	(7) Background Test Score and Attitudes	(8) Background, Test Score and All Personality Measures	(9) Background, Test Score and Significant[a/] Personality Measures except Perception of Ability	(10) Background Test Score and Significant Personality Measures
Academic Response					
Study Habits	[.035]		[.072]	.077	*
Best Work	[-.071]		-.088	-.094	-.088
Leadership Roles	.114		.098	.099	[illegible]
Never Gone Steady	-.107		-.093	-.098	-.096
Intellectual Reading	-.074		-.097	-.092	-.110
Attitudes					
Importance of Ins.	[.041]	[.006]	[-.011]	*	*
Educ. Necessary	[.058]	[.038]	[.043]	*	*
Work Orientation:					
Material	[.055]	[.001]	[-.003]	*	*
Interest	.072	[.056]	[.047]	*	*
Advancement	[.053]	[.008]	[.001]	*	*
R^2			.062	.082	
Perception of Ability			.112		.130
R^2			.069		.090

Notes:

Sample 843 individuals with complete data on background controls, Test Score, High School Curriculum, Years of Education, Hourly Earnings, as well as the 10 indirect measures of personality listed in the table.

a/ Perception of Ability was not entered in column 9 regression, so that except for controlling Test Score, this regression is identical to that reported in column 5.

* Entered if significant.

successful economically than others. Controlling cognitive ability increases the positive effect of going steady (Column 6), as does controlling Years of Education. It thus appears that students who have gone steady have lower academic ability, and obtain less education, but possess some other characteristic that enhances their earnings despite these disadvantages.

The negative coefficient of Intellectual Reading indicates that intellectuals have lower earnings at age 28. This may be because intellectuals trade income for intellectual challenge or other job characteristics. This disadvantage may also be only temporary. Intellectuals may choose jobs which pay less initially, but provide increased earnings later on.[13]

Alternatively, high school intellectualism may be negatively associated with later earnings because it reflects a rejection of the adolescent subculture. Following Coleman's (1961) argument, this may indicate a lack of concern for collective goals. Such individuals may thus be less productive than others on jobs that require them to adopt group goals and perform in a group context.

Never having gone steady and doing nonrequired reading have negative effects despite the fact that they are both positively related to socioeconomic background and academic ability. Individuals from advantaged backgrounds and with high ability thus have certain characteristics that depress their earnings, at least at 28.

13/ I also considered the possibility that intellectuals took longer to obtain any given level of education and had lower earnings because they had less job experience. However, when I controlled for Work Experience, the negative coefficient of Intellectual Reading declined less than 5 percent.

Comparing Columns 4 and 8 indicates that the effects of noncognitive traits on earnings are not generally tied to academic ability. The greatest decline is for Study Habits, indicating that students with good study habits have an advantage partly because they have greater academic ability.

Entered together after background characteristics, the noncognitive traits raise the explained variance in Hourly Earnings from 0.022 to 0.054. If we assume that academic ability precedes these measures, the contribution to explained variance remains appreciable, with $\bar{R}^2$ increasing from 0.032 to 0.062.

Although I have not shown them, regressions which predict Ln Hourly Earnings are very similar to those predicting Hourly Earnings. Background and noncognitive measures explain slightly less variance ($\bar{R}^2$ with background controls is 0.003 less, $\bar{R}^2$ adding noncognitive characteristics is 0.006 less). There are no important substantive differences.

3. Effects of All Talent Personality Measures

While our evidence indicates that the noncognitive traits affecting Education, Occupation and Hourly Earnings are numerous and only loosely related, it is nonetheless instructive to combine them into a single measure. We can do this by weighting each variable by its unstandardized coefficient and summing to form a new variable. Tables 5.20 to 5.22 present coefficients for such combined variables. The new variable labelled Noncognitive Traits is constructed from both self-assessed and indirect personality measures. The noncognitive characteristics embodied in this variable change from one regression to the next, since the noncognitive measures are reweighted in each regression to have maximum predictive power. Family background and cognitive ability also change in this way. This means that the correlations among traits also change from regression to regression, though the tables show that the changes are **seldom large.**

I have also entered several social psychological variables similar to those used by Sewell and Hauser (1972). These include Parents' Educational Hopes, Friends' Educational Plans, and respondents' Educational Plans and Occupational Preferences. Table 5.19 lists the questions and coding of these measures.

Effects on Education

A third of the effect of background on education works through personality traits. Controlling for background characteristics, the noncognitive composite has a standardized coefficient of 0.473. Controlling for academic ability decreases the apparent importance of noncognitive factors appreciably. Regression 3 indicates a direct effect of noncognitive factors represented by a standardized coefficient of 0.341, implying that 28 percent of the effect of the noncognitive factors is due to their association with academic ability. If cognitive ability is formed before these noncognitive traits, this proportion should be considered spurious. If cognitive ability develops after these noncognitive traits, 28 percent of their effect is traceable to the fact that they influence ability.

Table 5.19 Questions and Codings for Talent Measures of Student Encouragement, Plans, Expectations and Preferences

Parents' Educational Hopes	How much education do your parents or guardians want you to have?	
	They don't care whether I stay in high school	11
	High school only	12
	Vocational school, business school or junior college	13
	A college degree	16
	Professional or graduate school	18
	No answer	13
Friends' Educational Plans	How much education are most of your friends planning to obtain?	
	They are planning to quit high school	11
	Complete only high school	12
	Vocational school, business school or junior college	13
	Four year college training	16
	Professional or graduate training	18
	No answer	13
Teachers' Influence on Education	How many times have you discussed the following with your teachers or school principal in the past year?	
	College Plans	
	None	0
	One	1
	Two	2
	Three	3
	Four	4
	Five or more	6

Table 5.19 (continued)

Occupational Preference Index	Each of 112 items indicates an occupation, which I coded to a Duncan score. Students responded to each in one of five categories.	
	I would like this very much	25
	I would like this fairly well	16
	Indifferent or don't know much about it	9
	I would dislike this a little	4
	I would dislike this very much	1
	The code is an average of the Duncan scores (D_i): weighted by the individual's response to each (pi): $\frac{\sum p_i D_i}{\sum p_i}$	
Educational Plans	What is the greatest amount of education you expect to have during your life?	
	I don't expect to finish high school	11
	Expect to graduate from high school	12
	Vocational, business school or junior college training	13
	Some (less than 4 years) regular college training	14
	Graduate from 4 year college	16
	Study for advanced college degree	18
	No answer	13.5

Table 5.20 Combined Coefficients for Regressions of Education on Family Background, Personality Traits, Test Score and Social-Psychological Measures: Talent Males

	Family Background	Noncognitive Traits a/	Score	Grades b/	Parents' Educational Hopes	Friends' Educational Plans	Educational Plans	Occupational Prefer.	$\bar{R}^2$
1	.507								.245
2	.329	.473							.430
3	.253	.341	.341						.496
4	.259	.309	.310	.122					.507
5	.206	.216	.248	.100	[.033]	[.056]	.143	.094	.527

Correlations

Regression 3

1	Family Background	1.000		
2	Noncognitive Traits	.330	1.000	
3	Test Score	.353	.396	1.000

Regression 4

1	Family Background	1.000			
2	Noncognitive Traits	.325	1.000		
3	Test Score	.366	.386	1.000	
4.	Grades	.181	.347	.353	1.000

Regression 5

1	Family Background	1.000			
2.	Noncognitive Traits	.219	1.000		
3.	Test Score	.359	.250	1.000	
4	Grades	.175	.219	.350	1.000

Sample is 732 individuals with complete data on all variables.

a/ The following measures were entered in order of contribution to explained variance until no unentered measure increased explained variance significantly: Sociability, Culture, Leadership, Self Control, Mature Personality, Study Habits, Affiliations (+), Affiliations (-), Leadership Roles, Age at 1st Date, Never Dated, Times Gone Steady, Times Out per Week, Hours Worked, Cultural Events, Hobbies, and Importance of Life Insurance.

b/ History and Social Studies Grades entered if statistically significant.

Individuals with high academic ability tend to have personality traits that help them obtain more schooling, as indicated by a correlation of 0.396 between the academic composite (and its square) and the noncognitive traits that affect education.

Regression 4 shows that noncognitive traits also have appreciable effects with grades controlled. Regression 5 indicates that controlling the influence of parents and friends and the student's own Occupational Preferences and Educational Plans lowers the coefficient of Noncognitive Traits substantially. But even with all these potentially prior influences controlled, the noncognitive traits still have a sizable coefficient (0.216). Thus we may conclude that certain noncognitive characteristics influence individual attainment independent of family background, cognitive ability, grades, peer group pressures, and educational plans in 11th grade.

Effects on Occupation

Since noncognitive factors have a substantial effect on educational attainment, which in turn has a large effect on occupational status, regressions that predict Occupation resemble those predicting Education. But Noncognitive Traits have an appreciable effect on Occupational status even with education controlled (see equations 7-9), with standardized coefficients ranging from 0.185 to 0.166. The noncognitive traits that affect occupational status independent of education are much less closely tied to cognitive ability or family background than the noncognitive traits that predict educational attainment (correlations

Table 5.21 Combined Coefficients for Regressions of Occupation on Family Background, Personality Traits, Test Score and Social-Psychological Measures: Talent Males

	Family Background	Noncognitive Traits[a/]	Test Score	Grades[b/]	Parents' Educational Hopes	Friends' Educational Plans	Educational Plans	Occupational Prefer.	Education	$\bar{R}^2$
1	.389									.138
2	.266	.418								.283
3	.194	.309	.313							.330
4	.192	.270	.297	.117						.335
5	.159	.217	.211	.086	[.009]	[.067]	[.058]	.150		.348
6	.124								.607	.413
7	.126	.186							.550	.440
8	.105	.168	.142	*					.504	.448
9	.098	.166	.110	*	[-.012]	[.035]	[.002]	.094	.473	.452

Correlations

Regression 3

		1	2	3
1	Family Background	1.000		
2	Noncognitive Traits	.177	1.000	
3.	Test Score	.343	.269	1.000

Regression 5

		1	2	3	4
1	Family Background	1.000			
2	Noncognitive Traits	.048	1.000		
3	Test Score	.339	.180	1.000	
4	Grades	.146	.192	.328	1.000

Regression 8

1	Family Background	1.000			
2	Noncognitive Traits	-.001	1.000		
3	Test Score	.194	.120	1.000	
4	Education	.176	.230	.557	1.000

Regression 9

1	Family Background	1.000			
2	Noncognitive Traits	-.042	1.000		
3	Test Score	.158	.124	1.000	
4	Education	.104	.232	.557	1.000

*Tested, but not statistically significant.

Sample is 732 individuals with complete data on all variables.

a/ The following measures were entered in order of contribution to explained variance until no unentered measure increased explained variance significantly: Social Sensitivity, Impulsiveness, Leadership, Mature Personality, Study Habits, Best Work, Affiliations (+), Affiliations (-), Leadership Roles, Never Dated, Doesn't Date, Times Out per Week, Hours Worked, Cultural Events, Hobbies, and Importance of Insurance.

b/ History and Social Studies Grades entered if statistically significant.

0.120 and -0.001).

Of the social-psychological variables, only Occupational Preferences has an effect on Occupation after Education is controlled. Almost none of the post-educational influence of noncognitive factors on status attainment is due to the fact that individuals with the "right" noncognitive traits prefer high status jobs. Over 20 percent of the post-educational impact of cognitive ability is due to such preferences (compare equations 8 and 9).

Effects on Hourly Earnings

Table 5.22 presents combined effects of noncognitive traits on Hourly Earnings. Regressions 1 to 3 indicate that no more than a sixth of the effect of background on earnings works through measured noncognitive traits, even if those traits are assumed to preceed cognitive ability. It appears, then, that the noncognitive traits we have measured are not critical in facilitating the conversion of parental advantages into earnings.

Adding cognitive ability to the model indicates that the noncognitive characteristics do not work through ability either. The correlation matrix for Regression 3 indicates that students who have noncognitive characteristics that boost earnings are not particularly likely to have high test scores (r = 0.101) or to come from more advantaged backgrounds (r = 0.070). This conclusion is strengthened if we consider only those noncognitive traits that exert effects independent of the social-psychological variables. Intercorrelations among combined

Table 5.22 Combined Coefficients for Regreggions of Hourly Earnings on Family Background, Personality Traits, Test Score and Social-Psychological Measures: Talent Males

	Family Back-ground	Noncog-nitive Traits[a/]	Test Score	Parents' Educa-tional Hopes	Friends' Educa-tional Plans	Educa-tional Plans	Occupa-tional Prefer.	Educa-tion	Occupa-tion	R^2
1	.225									.036
2	.189	.286								.108
3	.153	.273	.138							.120
4	.125	.242	.090	[.018]	.105	[.044]	.103			.130
5	.119	.245	[.058]	[.019]	.090	[.014]	[.082]	.147		.140
6	.115	.245	[.042]	[.023]	.084	[.011]	[.071]	.098	.142	.150

Correlations

Regression 3

1	Family Background	1.000		
2.	Noncognitive Traits	.070	1.000	
3	Test Score	.265	.101	1.000

Regression 4

1	Family Background	1.000		
2	Noncognitive Traits	-.015	1.000	
3	Test Score	.171	-.099	1.000

Regression 6

1	Family Background	1.000				
2	Noncognitive Traits	-.015	1.000			
3	Test Score	.108	-.104			
4	Education	.069	-.040	.306	1.000	
5.	Occupation	.083	-.047	.463	.317	1.000

Sample is 732 individuals with complete data on all variables.

a/ The following measures were entered in order of contribution to explained variance until no unentered measure increased explained variance significantly. Leadership, Study Habits, Best Work, Leadership Roles, Never Gone Steady, Academic Reading, and Education Necessary. In those regressions that controlled Test Score, Perception of Ability was also entered, but it was never statistically significant.

measures for regression 4 indicate that individuals with greater cognitive ability are actually somewhat less likely to have personality characteristics that help them to obtain income. Similarly, men whose noncognitive characteristics increase their earnings at age 28 have no more schooling and no higher status occupations than the average. Thus the total effect of noncognitive traits remains appreciable (0.245) even after controlling Education and Occupation.

Part of the impact of the measured noncognitive traits on earnings **could** be due to the fact that 28 year olds with certain traits have different amounts of labor force experience.[14] **However, when I added Work Experience to the earnings equations, the coefficients for the non-cognitive measures (considered one at a time)** changed less than 10 percent. The coefficients usually fell, suggesting that individuals with favorable personality traits have slightly more labor force experience at 28 than others with similar education. But this does little to explain why they make more money.

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[14] In the simplest case, individuals with certain personality characteristics may have less experience, but only because they obtained more schooling. Controlling for education would account for this difference.

It is hard to be sure whether the effects of noncognitive traits will increase or decrease as the sample ages.

Interactions and Nonlinearities

The analyses presented up to this point have entered squared terms to account for nonlinearities in the effects of background characteristics and cognitive ability, and product terms to account for interactions between background characteristics. In contrast, the noncognitive traits have been treated as if their effects were exclusively linear and additive. To see if there were important nonlinearities, I added squared terms to the linear equations. Several were statistically significant, but none altered $\bar{R}^2$ enough to be substantively interesting. Since the original scaling of the noncognitive traits is arbitrary, the nonlineariites have no clear substantive interpretation

The scanty available evidence also indicates that personality traits may not have additive effects (see Rosen, 1971; Elder, 1968; and Crockett, 1962). Although each of these studies had serious flaws, they all suggested that individual motivation may be more important for individuals from lower status backgrounds. This would imply an interaction between family background and motivation. Similarly, if one personality trait either facilitates or blocks the effects of another, this would imply an interaction between the measures of personality. Numerous other potential interactions suggest themselves. We may suspect that individuals with certain personality characteristics will realize greater returns to ability or education than others. Gasson, Haller and Sewell (1972) suggested that student educational plans and occupational aspirations might interact positively with other advantages such as background and ability. This certainly seems reasonable, since a student should have less trouble realizing his aspirations or plans if his resources are greater.

To test such possibilities, I added a variety of product terms involving the personality measures to the additive regressions. Table 5.23 summarizes the results for the regressions predicting Education. The interactions I did not test are blank. An asterisk means that the interaction was not significant at the 0.05 level (i.e. $F < 3.86$). If the interaction was significant, I have shown its sign and F-level. The coefficient of the Socioeconomic Index X Sociability interaction is negative, for example, and its F is 4.1. This suggests that individuals from high status backgrounds suffer greater declines in educational attainment when they are sociable than do those from low status backgrounds.

Since Table 5.23 presents 102 terms, we expect at least five to be significant at the 0.05 level. Since 9 terms are actually significant at this level, half of the apparently significant interactions are

Table 5.23 Sign and F-level for Coefficients of Multiplicative Interactions Involving Personality Traits in Regressions Predicting Education: Talent Males

	Sociability	Culture	Leadership	Self-Confidence	Mature Personality	Study Habits	Best Work	Affiliations (+)	Hours per Week Job	Intellectual Reading	Education Necessary	Leadership Roles	Parents' Ed. Hopes	Friends' Ed. Plans	Educational Plans	Occupational Preferences
Father's Education													*	-4.9	*	*
Socioeconomic Index	-4.1	*	*	*	*	*	*	-5.6	*	*	*		*	*	*	*
White	*	*	*	*	*	*	*	*	*	+5.8	*		*	*	*	*
Test Score	*	*	*	*	+4.0	+6.1	*	*	*	*	*		*	*	+5.3	*
Sociability																
Culture	*															
Leadership	*	*														
Self-Confidence	*	*	*													
Mature Personality	-4.9	*	*	*												
Study Habits																
Best Work						*										
Affiliations (+)						*	*									
Hours per Week on Job						*	*	*								
Intellectual Reading						*	-5.0	*	*							
Education Necessary						*	*	*	*	*						
Leadership Roles																
Parents' Ed Hopes																
Friends' Ed Plans													*			
Educational Plans													*	*		
Occupational Pref.													*	*	*	

* $F < 3.86$ so $p > .05$

Statistics derived from several different regressions using slightly different samples (N   700).

Table 5.24 Sign and F-level for Coefficients of Multiplicative Interactions Involving Personality Traits in Regressions Predicting Occupation: Talent Males

	Social Sensitivity	Impulsiveness	Leadership	Mature Personality	Study Habits	Best Work	Affiliations (+)	Intellectual Reading	Education Necessary	Leadership Roles	Parents' Ed. Hopes	Friends' Ed. Plans	Educational Plans	Occupational Preferences
Father's Education											*	-7.3	*	*
Father's Occupation											*	*	*	*
Socioeconomic Index	-7.8	*	*	*	*	*	*	*	*		*	-4.5	*	*
White	-4.9	*	*	*	*	*	*	+6.4	*		*	*	*	*
Test Score	*	*	*	*	*	*	*	*	*		*	*	*	*
Years of Education	*	*	*	*	*	*	*	*	*		*	*	*	*
Social Sensitivity														
Impulsiveness	*													
Leadership	*	*												
Mature Personality	*	*	*											
Study Habits														
Best Work					*									
Affiliations (+)					*	*								
Intellectual Reading					*	*	*							
Education Necessary					*	*	*	*						
Leadership Roles														
Parents' Ed. Hopes				*	*					*				
Friends' Ed. Plans				*	*					*	*			
Educational Plans				*	*					*	*	*		
Occupational Pref.				*	*					*	*	*	*	

* F < 3.86 so p > .05

Statistics derived from several different regressions using slightly different samples. (N 700).

Table 5.25 Sign and F-level for Coefficients of Multiplicative Interactions Involving Personality Traits in Regressions Predicting Hourly Earnings: Talent Males

	Leadership	Study Habits	Best Work	Leadership Roles	Intellectual Reading	Education Necessary	Parents' Ed. Hopes	Friends' Ed. Plans	Educational Plans	Occupational Preferences
Father's Occupation		*	*	*	*		*	*	*	*
Socioeconomic Index	*	*	*	*	*	*	*	*	*	*
White	*	*	*	*	+10.1	*	*	*	*	*
Test Score	*	*	*	+ 4.3	*	*	*	*	*	*
Years of Education	*	*	*	*	*	*	*	*	*	*
Occupation	*						*	*	*	*
Leadership										
Study Habits										
Best Work		*								
Leadership Roles		*	*							
Intellectual Reading		*	*	*						
Education Necessary		*	*	*	*					
Parents' Ed. Hopes							*			
Friends' Ed. Plans							*	*		
Occupational Pref.							*	+ 5.4	*	

* F $<$ 3.86 so p $>$ .05

Statistics derived from several different regressions using slightly different samples (N 700).

probably spurious. Since I see no convincing rationale for the few significant effects, I will not discuss them individually.

Tables 5.24 and 5.25 present similar summaries of interaction terms for regressions predicting Occupation and Hourly Earnings. Again, most of the "significant" interactions are probably due to chance, so I will not discuss them.

The paucity of significant interactions is itself surprising. One would expect students from high status backgrounds, for example, to be more likely to obtain whatever schooling they plan and to end up in the jobs they prefer. Yet the interactions of Educational Plans and Occupational Preferences with background characteristics are never statistically significant.

In addition to testing the coefficients of interaction terms, I also performed numerous experiments with groups of interactions in various regressions. The contribution to explained variance of groups of interaction terms was often greater than would be expected by chance, suggesting that interaction effects exist. The explanatory power of the interactions was invariably small, however, and their inclusion did not change the coefficients for causally subsequent variables. Given the small sample and large sampling errors involved, substantive interpretation would be unwise.

I conclude that use of a linear, additive model does not seriously bias substantive conclusions. But this negative finding is only valid in the context of existing theory. Any comprehensive *a priori* theory that predicted the specific interactions I found would receive powerful support from these data. It is only in the absence of such a theory, and in the absence of empirical support for the obvious a priori theories discussed above, that I discount the interactions. Furthermore, even though I considered numerous potential interactions, it is possible that I overlooked some that were important. All I can say with certainty is that existing theories that predict specific interactions must be viewed skeptically.

4. Assessments of Personality by Others[15]

The Kalamazoo sample allows us to examine personality ratings by an individual who observes the student in high school, namely his teacher. Olneck (1976) obtained the public high school records of brothers who had been in the sixth grade in the Kalamazoo, Michigan, public school system between 1928 and 1950. He attempted to contact these individuals in 1973 and 1974, obtaining interviews with 1243 men aged 35 to 59. Appendix I gives a more complete description of the

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[15] My analyses below which control only measured background characteristics are based on correlations presented by Olneck (1976, Chapter 5). My estimates of effects which use brothers to control unmeasured family characteristics are based on my own analyses of Olneck's data. Olneck's treatment of this data differs from mine in that he considers in more detail the ways in which unmeasured personality characteristics may bias estimated returns to cognitive ability and schooling.

Kalamazoo sample. Kalamazoo's tenth grade homeroom teachers had rated their students on nine character traits, labeled Cooperativeness, Dependability, Executive Ability, Emotional Control, Industriousness, Initiative, Integrity, Perseverance, and Appearance. They rated each student Above Average, Average, or Below Average on each trait. Different teachers may have interpreted these nine traits differently. Homeroom teachers were in charge of as many as 80 students, and I do not know how many of these students they had taught in academic classes. The ratings may therefore have been partly based on second hand information and on the general reputation of the student within the school.

More than half the students received ratings of Average on each of the nine traits. The proportion of students rated Above Average ranged from 32.1 percent for Cooperativeness to 10.8 percent for Executive Ability. In general, teachers rated fewer students Below Average than Above Average on any trait. The exception was Executive Ability, with more than twice as many students (24.7 percent) rated Below Average as Above Average. I first coded these responses as an equal interval scale (Below Average = 1/Average = 2/Above Average = 3). As expected, higher ratings were positively associated with later achievement, and the equal-interval scale captured the dominant effect. But when I used dummies for each category, the difference between Above Average and Average was greater than the difference between Average and Below Average. The ratio of the two increments was sometimes as high as 4:1. This difference may reflect an actual nonlinearity in the effect of such character traits. Alternatively, teachers may make more accurate distinctions at the upper end of the scale than at the lower end. Although deviations from linearity were consistent and often statistically significant, they were not large enough to alter substantive conclusions. Furthermore, when individual background characteristics and cognitive ability were controlled in the regression, deviations from linearity were generally smaller and seldom statistically significant.[16] I therefore retained the equal-interval coding in the analyses which follow.

Table 5.26 provides correlations among teacher ratings and other variables for the Kalamazoo sample. The different teacher ratings are closely associated with one another, intercorrelations being at least 0.4. This may be because the underlying character traits tend to vary together, because teachers tend to rate individuals consistently without regard to the actual pattern of traits, or because different ratings measure the same underlying trait. Correlations of Dependability with **Cooperativeness** and of Perseverance with Industriousness exceed 0.8, suggesting that these traits are perceived as very similar. The correlations of the teacher ratings with cognitive ability are consistently smaller (0.2 to 0.3) than the correlations among the ratings. If teachers are rating students largely on an underlying unitary trait, that trait is not closely associated with cognitive ability.

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[16] These analyses were done by Olneck (1976).

Table 5.26 Correlations Among Measured Family Characteristics, Test Score, Teacher Ratings and Dependent Variables: Kalamazoo Males

	1	2	3	4	5	6	7	8	9	10
1. Father's Education	1.000									
2. Father's Occupation	.428	1.000								
3. Siblings	-.223	-.161	1.000							
4. Test Score	.232	.190	-.151	1.000						
5. Cooperativeness	.223	.153	-.086	.212	1.000					
6. Dependability	.196	.154	-.081	.261	.824	1.000				
7. Executive Ability	.165	.067	-.064	.254	.428	.419	1.000			
8. Emotional Control	.222	.119	-.054	.217	.638	.644	.482	1.000		
9. Industriousness	.222	.189	-.024	.215	.670	.697	.429	.625	1.000	
10. Initiative	.221	.125	-.050	.300	.498	.466	.610	.490	.547	1.000
11. Integrity	.210	.160	-.036	.218	.703	.749	.367	.645	.597	.418
12. Perseverance	.177	.147	-.069	.197	.711	.700	.443	.636	.817	.534
13. Appearance	.206	.155	-.172	.214	.471	.446	.356	.493	.423	.418
14. Education	.407	.283	-.226	.478	.358	.377	.313	.334	.364	.290
15. Early Occupation	.332	.337	-.177	.432	.286	.322	.276	.252	.337	.237

Table 5.26 (continued)

	1	2	3	4	5	6	7	8	9	10
16. Current Occupation	.234	.221	-.186	.402	.208	.242	.240	.212	.301	.256
17. Earnings	.203	.158	-.114	.310	.177	.149	.262	.147	.181	.219
18. Lnearnings	.183	.156	-.106	.332	.162	.138	.250	.141	.180	.216

	11	12	13	14	15	16	17	18
11. Integrity	1.000							
12. Perseverance	.603	1.000						
13. Appearance	.462	.462	1.000					
14. Education	.328	.365	.311	1.000				
15. Early Occupation	.216	.308	.229	.729	1.000			
16. Current Occupation	.198	.288	.237	.586	.573	1.000		
17. Earnings	.135	.163	.200	.437	.424	.454	1.000	
18. Lnearnings	.123	.152	.199	.437	.431	.491	.961	1.000

Sample 389 Kalamazoo respondents with complete data on Father's Education, Father's Occupation, Siblings, Test Score, Education, Early Occupation, Earnings and the 9 teacher ratings.

Determinants of Teacher Ratings

Table 5.27 presents regressions of the character ratings on father's education, father's occupation and family size. Individuals whose fathers have more schooling obtain better ratings from teachers, while those with other family advantages generally do not. The association between the ratings and family background is not very strong. The maximum $\bar{R}^2$ for any rating is 0.056. If these three family characteristics captured all aspects of the family that made brothers alike, the correlations between brothers' ratings would be equal to the $\bar{R}^2$ from these regression equations. In fact, the correlations between brothers' teacher ratings are much higher, ranging from 0.237 to 0.461 (see Table 5.27). These correlations may not be completely due to shared family environment, since teachers may rate a student partly on a basis of his brother's behavior. Brothers may also affect one **another's** behavior, making them more alike than would be expected on a basis of their common background. It seems unlikely, however, that these factors alone would explain the large **sibling** correlations. Probably brothers share other genetic or environmental influences not measured by father's education and occupation or family size.

Effects on Education

Table 5.28 indicates that after controlling measured background and cognitive ability in 6th grade, individuals with favorable teacher ratings obtain more schooling. This is best seen as a single "teacher approval" effect. The first principal component of the teacher ratings

Table 5.27 Regression of Teacher Ratings on Measures of Family Background and Proportion of Variance Explained by Unmeasured Family Characteristics: Kalamazoo Males

	Individual Sample				Brother's Sample	
	Father's Education	Father's Occupation	Siblings	$\bar{R}^2$	Measured Family $\bar{R}^2$	All Family $\bar{R}^2$
Cooperativeness	187	[.068]	[-.033]	.047	.050	.409
Dependability	.153	[.083]	[-.034]	.038	.048	.454
Executive Ability	.161	[.007]	[-.029]	.020	.018	.237
Emotional Control	.209	[.029]	[-.003]	.043	.037	.455
Industriousness	.179	.117	[.035]	.054	.051	.459
Initiative	.205	[.037]	[.002]	.043	.066	.255
Integrity	.177	[.087]	[.017]	.043	.054	.461
Perseverance	.135	[.085]	[-.025]	.031	.025	.471
Appearance	.147	[.072]	-.128	.056	.037	.423

Samples Individual Sample is 389 respondents with complete data as specified in Table 5.26.

Brother's Sample is constructed from pairs of brothers who both have data on Test Score, Education, Early Occupation, Earnings, the nine teacher ratings, and for whom at least one brother reported on Father's Education, Father's Occupation and Siblings (210 individuals, 105 pairs).

Table 5.28 Regressions of Education on Teacher Ratings Taken One at a Time Controlling Family Characteristics and Test Score: Kalamazoo Males Aged 35-59

Standardized Coefficients	Individuals Sample (N=389)	Brothers Sample (N=210)	
Cooperativeness	.214	.223	.207
Dependability	.224	.230	[.155]
Executive Ability	.173	[.078]	[-.011]
Emotional Control	.102	.140	[.029]
Industriousness	.235	.243	.210
Initiative	.1[illegible]4	[.074]	[.093]
Integrity	.137	.168	[.162]
Perseverance	.238	.279	.307
Appearance	.159	[.102]	[.057]
Principal Component	.260		
Controls			
Measured Family Background[a]	X	X	
Characteristics Common to Brothers[b]			X
Test Score	X	X	X
$\bar{R}^2$ Controls Only	.329		
$\bar{R}^2$ with Principal Component	.389		

Samples as specified in Tables 5.26 and 5.27.

[a] Father's Education, Father's Occupation and Siblings

[b] Characteristics common to brothers were controlled by regressing difference in brothers' educations on differences in teacher ratings, controlling differences in Test Score.

explains educational attainment as well as the nine separate ratings. It increases $\overline{R}^2$ from 0.329 to 0.389.

Table 5.42 also shows the effects of teacher ratings in the smaller sample which contains pairs of brothers. When all the characteristics brothers have in common are controlled, the effects of the teacher ratings decline by an average of about 20 percent. However, even in this small sample, Cooperativeness, Industriousness and Perseverance remain statistically significant. It therefore appears that effects of these traits on Education are only partly due to the unmeasured characteristics that brothers have in common.

Effects on Occupation

Table 5.29 indicates that positive ratings by teachers are associated with higher status first occupations, after family characteristics and cognitive ability are controlled. However, the principal component does not adequately reflect the effects of the separate ratings. Indeed, if we enter measures together (column 2), their effects are no longer even uniformly positive. Initiativ and Integrity have negative effects on outcomes. Experiments with various forms of this regression indicate that if we omit Dependability, Integrity no longer has an appreciable negative effect. This suggests that individuals rated as having integrity but **not as** dependable suffer on the job market. This negative effect is generally hidden, since individuals are usually rated similarly on both traits. A similar relationship holds between Executive Ability and Initiative, suggesting that taking initiative

Table 5.29 Regression of Early Occupation on Teacher Ratings Taken One at a Time Controlling Family Characteristics and Test Score: Kalamazoo Males Aged 35-39

	Individuals Sample (N=398)				Brothers Sample (N=210)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cooperativeness	.155		[.017]		.162	[.016]	.215	[.082]
Dependability	.182	.153	[.039]		.214	[.067]	.253	[.154]
Executive Ability	.157	.132	[.046]		[.108]	[.058]	.188	.195
Emotional Control	.125		[.001]		.124	[.025]	[.118]	[.099]
Industriousness	.207	.182	[.066]	.125	.229	[.074]	.305	.175
Initiative	[.079]	[-.103]	[.006]		[.036]	[-.013]	[.026]	[.088]
Integrity	[.080]	-.138	[-.043]	-.109	[.056]	[-.058]	[-.023]	[-.134]
Perseverance	.191		[.039]		.213	[.031]	.388	.206
Appearance	.090		[-.014]		[.092]	[.024]	[-.024]	[.062]
Controls								
Measured Family Background	x	x	x	x	x	x		
All Background							x	x
Test Score	x	x	x	x	x	x	x	x
All Significant Teacher Ratings		x		x				
Education			x	x		x		x
$\bar{R}^2$ Controls only		.271		.552				
$\bar{R}^2$ Significant Traits		.324		.561				

a/ Columns 2 and 4 show coefficients for regressions in which I entered the traits in order of contribution to explained variance, until no unentered trait had a statistically significant effect. Hence, these regressions control for all significant teacher ratings. Other columns present coefficients for regression which enter traits one at a time, i.e., no other traits are controlled.

Samples are as specified in Tables 5.26 and 5.27.

Table 5.30 Regression of Occupation on Teacher Ratings Taken One at a Time Controlling Family Characteristics and Test Score: Kalamazoo Males Aged 35-59

	Individuals Sample				Brothers Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cooperativeness	.099		[.009]	-.111	.143	[.034]	[.025]	[-.064]
Dependability	.121		[.009]		.193	[.084]	[.072]	[.008]
Executive Ability	.135		[.050]		[.066]	[.027]	[-.001]	[.003]
Emotional Control	.108		[.012]		.130	[.058]	[.011]	[-.001]
Industriousness	.203	.203	.097	.165	.273	.165	.318	.243
Initiative	.130		[.074]		.156	[.119]	[.027]	[.012]
Integrity	[.091]		[-.003]		.121	[.038]	[.019]	[-.049]
Perserverance	.194		[.081]		.291	.171	.397	.300
Appearance	.123		[.044]		.153	.103	[.032]	[.009]
Controls								
Measured Background	x	x	x	x	x	x		
All Background							x	x
Test Score	x	x	x	x	x	x	x	x
All Significant Teacher Ratings		x		x				
Education			x	x		x		x
$\bar{R}^2$ Controls Only		.193		.360				
$\bar{R}^2$ Significant Traits[a]		.229		.371				

Sample as specified in Tables

[a] Columns 2 and 4 show coefficients for regressions in which I entered the traits in order of contribution to explained variance, until no unentered measure had a statistically significant effect.

is not in itself helpful, but appears helpful because those who take initiative are more likely to have other abilities.

Controlling for years of schooling reduces the effects of all traits appreciably. Considered separately, no teacher rating has a statistically significant impact on early occupation after controlling education. If we enter ratings together, Industriousness appears to have a positive effect and Integrity a negative effect. The contribution to explained variance is minimal, however.

Analysis on the smaller brothers' sample yields similar results when we control measured background and cognitive ability. (Although Columns 5 - 8 only show effects of ratings considered separately, regressions which control more than one teacher rating are also similar to those using the larger sample.) Controlling all characteristics which brothers have in common, however, increases the apparent effects of the ratings. After controlling all characteristics brothers have in common, as well as cognitive ability and education, three ratings have statistically significant effects on early occupational status, whereas none had an effect when we controlled only measured family characteristics.

Table 5.30 indicates that effects of teacher ratings on occupational status are weaker in maturity than at the start of men's careers. After controlling education, however, effects on mature status are at least as great as on initial status. Regressions of mature occupational status in the smaller sample in which I can control shared family characteristics are much like those predicting early occupation. After controlling for difference in ability and schooling, personality differences between brothers appear more important than personality differences between individuals from similar demographic backgrounds.

The increase in the coefficients of most personality traits when we look at differences between brothers suggests that the unmeasured background characteristics that affect personality traits must be negatively correlated with those that affect occupational status. **This is rather puzzling, since those measured family characteristics that affect teacher ratings are much like those that affect adult success. In any event, these findings suggest that the** modest effects of teacher character ratings on occupational achievement would not disappear if family environment had been measured more carefully.

Since effects of teacher ratings on early job status are similar to their effects on later status, after ability and schooling are controlled, we may conclude that **these traits have a continuing influence on men's achievements.** 17/ The sample is too small--especially in analyses which consider brothers--to determine the relative importance of these traits, but it is clear that effects exist.

Effects on Earnings

Table 5.31 indicates that the teacher ratings generally have positive effects on earnings after controlling measured family background and ability. Effects are appreciably smaller after schooling is controlled,

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17/ Regressions that control early occupational status, in addition to cognitive ability, and educational attainment predicting later status suggest that these traits may affect later achievement directly, although such effects are probably small. This would suggest that whatever traits teacher ratings capture continue to affect the behavior of individuals through middle age.

Table 5.31 Regression of Earnings on Teacher Ratings Taken One at a Time Controlling Family Characteristics and Test Score: Kalamazoo Males Aged 35-59

	Individuals Sample (N=398)	
Cooperativeness	[.091]	[.015]
Dependability	[.051]	[-.033]
Executive Ability	.184	.126
Emotional Control	[.061]	[-.009]
Industriousness	[.096]	[.016]
Initiative	.119	[.080]
Integrity	[.047]	[-.021]
Perseverence	[.085]	[-.001]
Appearance	.115	[.059]
Controls		
Family Background	x	x
Test Score	x	x
Education		x
Early Occupation		
Occupation		
$\bar{R}^2$ Controls only	.109	.195
$\bar{R}^2$ with Executive Ability[a/]	.139	.207

Sample is as specified in Table 5.26.

[a/] After Executive Ability, no other measure had a significant effect on earninings. Controlling Occupation decreased the coefficience for Executive Ability to 0.113, and controlling also Early Occupation decreased it to 0.108.

with only the effect of Executive Ability being statistically significant. Controlling for Occupation decreases the standardized coefficient of Executive Ability to 0.108 (regression not shown).

Regressions with the smaller sample of brothers reveal little of interest and are therefore not shown.

No teacher rating has a statistically significant effect on Earnings in any regression in this small sample, regardless of whether the ratings are considered separately or together.

The regressions of Ln Earnings on teacher ratings do not differ appreciably from those of Earnings.

Conclusion

Given the circumstances under which teachers rated students, and the crude scale they used, the moderate impact of these ratings on individual achievement is surprising. The effects clearly persist into middle age, and analyses using brothers indicate that effects would persist even if family environment were measured in more detail. It seems safe to conclude that at least some noncognitive characteristics are formed by the time an individual reaches high school and that they remain stable enough in later years to influence individual achievement.

Overall Conclusion

My findings support the notion that individuals possess stable personality characteristics that influence their occupational and economic success. High school students with certain traits obtain higher status jobs and greater earnings than do others, even after controlling for family background and cognitive ability.

I found little support, however, for theories which suggest that a single personality trait is of critical importance in determining individual success. **Rather, each trait that influences success has a small effect.** Only when the effects of these various personal characteristics are considered together do they explain even a moderate portion of the observed differences in individual achievement. In general, the personality characteristics that predict success are neither closely tied to family status, nor to cognitive ability.

Contrary to expectations based on extent theory, my analyses failed to find any consistent or important interactions between personality traits and family background, test scores, or education.

Chapter 6

THE EFFECTS OF EDUCATION

by Michael Olneck[1]

Section 1: Introduction

Men with more schooling have higher status jobs and earn more money than men with less schooling. Public policy recognizes these facts by according educational programs significant importance in efforts to extend economic opportunity to the disadvantaged and reduce poverty.[2] Commonplaces like "Get an education" and "Stay in school" reflect the popular faith in the economic importance of schooling.

This chapter is concerned principally with the extent to which the apparent economic advantages of lengthier schooling are due to the characteristics of better educated men which affect both educational attainment and economic success. If men who get a lot of schooling possess characteristics which would lead to economic advantage even in the absence of educational advantage, the apparent benefits of schooling are likely to exceed the actual benefits. If men who do not persist in their schooling were to acquire more schooling they might well be disappointed in their expectations of realizing economic gain from their educational accomplishments.[3]

The chapter is concerned also with the extent to which the economic advantages associated with lengthier schooling vary by level of schooling, and by racial, social origin, age, and cognitive classifications. If public policy seeks to enhance economic opportunity by extending educational opportunity, it is important to know if all increments in schooling promise the same benefits or if there are levels of schooling whose effects are unusually large or robust. Conversely, it is important to know if there are levels of schooling whose effects are minimal or unusually biased by failure to control economically and educationally relevant characteristics.

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[1] In addition to my colleagues on this project, I am grateful to John Bishop and Robert Hauser for comments on an earlier draft of this chapter.

[2] See Levin (forthcoming) for discussion of the educational programs operating under the War on Poverty rubric.

[3] For technical treatments of the problem of bias due to omitted variables, see Goldberger and Duncan (1973). For discussion of the sources of the relationship between schooling and income, see e.g. Becker (1964), Taubman and Wales (1972), and Thurow (1972, 1975).

Estimates of the effects of education which are true "on the average" may vary among subgroups. Policies based on relationships estimated over the general population may consequently be misguided if they are directed toward atypical target populations. I have therefore reported relationships among economic outcomes and schooling separately for nonwhites and whites, sons of white-collar and blue-collar fathers, men with high, medium, and low test scores, and men from different age cohorts.

Throughout most of the chapter I am concerned with the effects of years of schooling. Ideally, we would want to measure quality of education as well as quantity. By and large this data is unavailable. But one of our data sets includes a measure of college quality, and another includes information on high school curriculum. **I discuss the effects of college quality and curriculum placement toward the end of the chapter. I first consider the effects of educational attainment on the status of the jobs men held early in their careers, then on their current occupations, and then on their current earnings or individual incomes.**

2 Early Occupation

Three of our data sets include information on the jobs men held directly after they completed their schooling. The OCG item is flawed, however, and we have ignored it throughout this study.[4/] Table 6.1- shows the effects of

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[4/] See Duncan, Featherman, and Duncan (1972, pp. 210-212) for a discussion of this item.

education on early occupational status for the Michigan Panel sample and for the Kalamazoo sample.

The average effect of an extra year of schooling on early occupational status in the Kalamazoo sample(6.238 points on the Duncan scale) is twice as large as the effect in the PSID sample (3.125 points). The difference is partly due to the fact that SRC only coded PSID respondents' occupations into nine broad categories. This reduces both the apparent variance in initial occupational status and the apparent effect of education on status. In addition, the PSID and Kalamazoo coefficients differ partly because the effects of education on initial occupational status are non-linear, and the Kalamazoo sample is drawn primarily from the upper end of the education distribution, where the effects of an extra year of education are greater.

Table 6.1 therefore presents several non-linear equations. These require brief explanation. Equation 2 includes three education measures: Years of Education (i.e. highest grade of school completed), a "spline" for Years of Higher Education (coded 0 to 6) and a dummy variable for having a BA (coded 0 or 1). The coefficient of Years of Education in equation 2 estimates the linear effects of a year of elementary or secondary education, i.e. 1.365 points. The coefficient of Years of Higher Education estimates the difference between the linear effect of a year of elementary or secondary education and the linear effect of a year of higher education, i.e. 2.669 points. The overall linear effect of a year of higher education is therefore the sum of the Years of Education coefficient and the Years of Higher Education coefficient, i.e., 4.032 points. The coefficient of BA represents the difference between the observed occupational status of men with 16 or more years of schooling and the status predicted on the assumption that the effects of higher education are linear, i.e. 6.986 points. The effect of the last year of college is therefore the sum of the Years of Education coefficient, the Years of Higher Education coefficient, and the BA coefficient, i.e. 11.018 points. If all causally prior variables were controlled, and if all years of higher education other than the fourth had the same effect on occupational status, the coefficient of BA would measure a "pure" credential effect. All causally prior traits are not in fact controlled, and since different years of higher education have somewhat different effects, the coefficient of BA should probably be interpreted merely as another non-linear effect.

There are educational advantages associated with both coming from more favorable home backgrounds and from displaying greater cognitive competence. There are also occupational advantages associated with variations in background and cognitive skill among men who have the same amount of schooling. If background and cognitive skills are ignored the apparent effects of education on early occupational status will be overestimated. The extent to which this is true, however, appears rather modest. Because the uncontrolled effects differ between the samples I will discuss both absolute and proportionate biases.

In the PSID sample, controlling test scores and measured family background reduces the effect of an extra year of schooling by 3.125 - 2.513 = 0.612 points or 0.612/3.125 = 19.6 percent. In the Kalamazoo sample, controlling test score differences among brothers and family background common to brothers reduces the effect of education by 6.238 - 5.526 = 0.712 points or 0.712/6.238 = 11.4 percent. These results suggest that

Table 6.1 Effects of Education on Early Occupational Status

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
PSID (N=1744)	1.	3.125 (.129)			**17.936**	None
	2.	1.363 (.212)	2.669 (.614)	6.986 (2.405)	**17.319**	None
	3.	2.677 (.160)			**17.733**	Measured Background[a]
	4.	.897 (.227)	2.457 (.620)	8.164 (2.396)	**17.121**	Measured Background[a]
	5.	2.862 (.146)			**17.866**	Test Score
	6.	1.014 (.224)	2.719 (.610)	7.211 (2.391)	**17.218**	Test Score
	7.	2.513 (.150)			**17.693**	Measured Background[a], Test Score
	8.	.690 (.234)	2.493 (.618)	8.222 (2.388)	**17.065**	Measured Background[a], Test Score
Kalamazoo Brothers (N=692 or 346 pairs)	9.	6.238 (.232)			**16.622**	None
	10.	3.166 (.701)	[1.295] (1.016)	15.137 (3.264)	**16.125**	None
	11.	5.710 (.264)			**16.377**	Measured Background[b]
	12.	2.389 (.718)	[1.710] (1.011)	14.274 (3.215)	**15.861**	Measured Background[b]
	13.	5.997 (.283)			**16.612**	Test Score
	14.	2.827 (.730)	[1.436] (1.019)	14.868 (3.264)	**16.105**	Test Score
	15.	5.520 (.303)			**16.366**	Measured Background[b]. Test Score
	16.	2.146 (.740)	[1.804] (1.013)	14.075 (3.217)	**15.851**	Measured Background[b]. Test Score

Table 6.1 Continued

Sample		Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
Kalamazoo Brothers (N=692 or 346 pairs)	17.	5.578 (.454)			15.465	Family Background[c]
	18.	[1.661] (1.661)	[2.614] (1.543)	13.787 (4.496)	15.025	Family Background[c]
	19.	5.526 (.488)			15.490	Family Background,[c] Test Score Difference
	20.	[1.580] (1.232)	[2.644] (1.547)	13.744 (4.503)	15.044	Family Background,[c] Test Score Difference

a. Race, Father's Education, Father's Occupation, Father White Collar, Father Foreign Born, No Male Head, Nonfarm origin, Non South origin, Number of siblings

b. Father's Education, Father's Occupation, Number of Siblings

c. **Family background controlled by defining education, test score, and occupation variables as sibling differences.**

when employers favor better-schooled young men they are either seeking characteristics that are not strongly related to cognitive ability and family background, or that they are bad judges of ability and background, and are forced to rely on educational credentials as an imperfect guide. Analyses of a Kalamazoo subsample for whom high school teachers' personality ratings are available suggest that similar conclusions hold when personality characteristics such as initiative or industriousness are considered as possible sources of bias. Inclusion of the personality measures does not significantly change the education coefficient.[5]

The extent to which increments in educational attainment are associated with higher occupational status in the early career varies by level of schooling. The sensitivity of the measured effects of schooling to the inclusion of background and ability measures also varies by level of schooling.

Increments in schooling below the college level are associated with smaller early occupational advantages than are increments at the college level, and they are reduced by a proportionately larger amount when test scores and background are controlled. In the PSID sample, the predicted advantage of a 12th grade graduate over an 8th grade graduate with the same test score and measured background is only 4(0.690) = 2.760 points, or 2.760/5.452 or 50.6 percent of the uncontrolled effect. In the Kalamazoo sample, the analogous effect among men who came from the same homes and have equal test scores is 6.320 points, or 6.320/12.664 = 49.9 percent of the uncontrolled effect.

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[5] See Olneck (1976), Chapter 5.

Four years of college, however, is associated with an extra 4(0.690 + 2.493) + 8.222 = 20.954 points among PSID respondents with equal test scores and similar backgrounds, and an extra 4(1.580 + 2.644) + 13.741 = 30.637 points among brothers with equal test scores in the Kalamazoo sample. **These effects are 90.7 percent and 92.9 percent of the zero-order effects in the PSID and Kalamazoo samples respectively.**

The substantial relative bias in the effects of schooling below the college level indicates that men who complete high school get better jobs than men who drop out in large measure because they are already advantaged. If this finding is accurate and if it holds for young men today, programs aimed at discouraging high school students from dropping out of school will not be likely to realize the full extent of their hopes of increasing the prospective dropout's employment prospects.

The robust effect of completing college suggests that either college augments employability for reasons unrelated to family background or cognitive skill, or that employers are less concerned with background and cognitive differences among college graduates. Since the economic impact of test scores increases over men's careers, we cannot conclude that employers are indifferent to cognitive skills. But since the impact of test scores on early occupational status is small after education is controlled, I conclude that employers either do not perceive or do not care about these cognitive differences when hiring BA's for entry level jobs in high status occupations. At the same time, most employers refuse to place men without degrees in high status occupations even when they have test scores as high or higher than typical college graduates.

3. Current Occupation

Effects of Controlling Family Background

Table 6.2 shows that the occupational advantages associated with additional schooling vary across our samples. This is because the data sets we are working with sampled different populations, had varying degrees of success in interviewing potential respondents, and coded important variables differently. For example, Project Talent followed up men who had at least entered 11th grade. In fact, 97 percent of the Talent respondents completed 12th grade.[6] The effect of educational variation in Talent is therefore the effect of progress through college and graduate school. A year of higher education has more impact on occupational status than a year of elementary or secondary education. Similarly, the PSID and PA surveys classified respondents' occupations into only nine broad categories. This reduces the variance of occupational status. It also reduces the measured effects of schooling but not to the same degree.

The effects of background and ability measures on education and economic outcomes also differ from sample to sample. In some cases, this is because coding differs. In the PSID and PA, SRC assigned missing values for Father's Education on the basis of reported literacy. Father's Occupation was coded only in broad categories. In other cases, sampling error produces differences. Among NORC Brothers, for example, the inter-correlations among background variables are slightly higher than those among the background variables in the OCG. In still other cases, sample restrictions are responsbible. The Talent and Veterans samples are selected in some measure on educational attainment, reducing the measured impact of background on education. The Kalamazoo sample may also be selected partly on current occupational status and earnings. In any case.

6_/ This figure applies to the Talent complete data sample described by Crouse in Appendix H. The Talent siblings analyzed here average 0.36 years more schooling than the Talent complete data respondents.

the effects of Father's Occupation are certainly lower in that sample than in nationally representative samples.[7/]

Because the uncontrolled effects of education are not the same across samples, and because the interrelations among measures of background, cognitive ability, schooling, and occupation vary, I cannot offer precise conclusions about the magnitude and sources of bias in the occupation-schooling relationship. I can, however, suggest the most important sources of bias and the levels of schooling which are most sensitive to controls for omitted variables.

Higher status families ensure that their sons will have greater than average chances of attaining economic success mainly by increasing their chances of getting a lot of schooling. However, measured family background is associated with occupational status even among men with the same amount of education. Consequently, the occupation-schooling relationship is somewhat overestimated when the effects of measured background are ignored.

Data from the OCG and NLS suggest that about a fifth of the apparent effect of education on occupational status is due to the joint association of education and occupation with measured background (See Table 6.2). The reductions in the education coefficient controlling background are 0.751 points in the OCG and 0.723 points in the NLS.

Because of occupational coding differences, the reductions in the Panel Study and Productive American

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[7/] See Olneck (1976), pp. 86-90.

Table 6.2 Effects of Education on Current Occupation

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1970 Census (N=25697)	1.	4.337 (.034)			19.243	None
	2.	2.934 (.055)	2.465 (.164)	4.013 (.770)	18.835	None
1962 OCG (N=11504)	3.	4.105 (.050)			19.731	None
	4.	2.701 (.073)	3.079 (.287)	5.163 (1.275)	19.138	None
	5.	3.354 (.058)			19.085	Measured Background[a]
	6.	1.988 (.079)	2.928 (.284)	5.710 (1.234)	18.500	Measured Background[a]
1972 PSID (N=1744)	7.	3.910 (.119)			16.567	None
	8.	2.134 (.195)	2.951 (.564)	5.546 (2.210)	15.916	None
	9.	3.579 (.139)			16.443	Measured Background[b]
	10.	1.684 (.209)	3.103 (.570)	6.001 (2.203)	15.743	Measured Background[b]
	11.	3.664 (.135)			16.502	Test Score

Table 6..2 Continued (2)

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
	12.	1.807 (.206)	2.997 (.560)	5.757 (2.197)	**15.819**	Test Score
	13.	3.438 (.148)			**16.410**	Measured Background,[b] Test Score
	14.	1.501 (.215)	3.136 (.560)	6.051 (2.197)	**15.696**	Measured Background,[b] Test Score
	15.	1.377 (.211)	2.685 (.560)	4.565 (2.162)	**15.394**	Measured Background,[b] Test Score, Early Occupation
1964 Productive Americans (N=1188)	16.	3.509 (.139)			**16.633**	None
	17.	2.105 (.204)	3.861 (.764)	[.534] (3.066)	**16.065**	None
	18.	3.148 (.163)			**16.398**	Measured Background[c]
	19.	1.669 (.221)	3.975 (.767)	[.778] (3.036)	**15.800**	Measured Background[c]
1966 NLS **Men Aged 45 to 59 (N=2830) (N=2830)**	20.	4.075 (.101)			**19.745**	None
	21.	2.896 (.143)	2.785 (.620)	5.490 (2.778)	**19.268**	None
	22.	3.352 (.117)			**19.077**	Measured Background[c]
	23.	2.079 (.155)	3.220 (.604)	[4.227] (2.693)	**18.563**	Measured Background[d]
1964 Veterans aged 30 - 34 (N=803)	24.	5.070 (.242)			**18.781**	None
	25.	1.889 (.439)	4.816 (.933)	[4.843] (3.580)	**17.945**	None
	26.	4.677 (.258)			**18.435**	Measured Background[e]
	27.	1.641 (.446)	4.472 (.929)	[5.438] (3.532)	**17.663**	Measured Background[e]
	28.	4.385 (.287)			**18.579**	Test Score

Table 6..2 Continued (3)

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1974 NORC Brothers (N=300 or 150 pairs)	29.	1.046 (.464)	4.851 (.919)	[5.511] (3.530)	17.679	Test Score
	30.	4.131 (.296)			18.292	Measured Background,[e] Test Score
	31.	.979 (.468)	4.466 (.919)	[6.069] (3.497)	17.475	Measured Background,[e] Test Score
	32.	4.634 (.363)			19.480	None
	33.	3.260 (.684)	2.117 (1.541)	[1.593] (6.770)	19.360	None
	34.	4.321 (.401)			19.114	Measured Background,[f] Age
	35.	2.676 (.747)	[2.871] (1.568)	[-2.008] (6.871)	18.952	Measured Background,[g] Age, Age^2
	36.	3.193 (.437)			17.967	Family Background,[k]
	37.	[1.457] (1.112)	[3.778] (2.127)	[-.223] (9.008)	17.854	Family Background,[k] Age Difference
1971-72 Talent Siblings (N=198 or 99 pairs)	38.	8.324 (.525)			18.214	None
	39.	7.307 (.595)			17.988	Measured Background[h]
	40.	6.912 (.678)			18.217	Test Score
	41.	7.098 (.713)			18.021	Measured Background,[h] Test Score
	42.	6.613 (1.091)			17.980	Family Background[k]
	43.	6.506 (1.206)			18.069	Family Background,[k] Test Score Difference

Table 6.2 Continued (4)

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1974 **Kalamazoo Brothers (N=692 or 346 pairs)**	**44.**	5.012 (.261)			**18.696**	None
	45.	5.722 (.809)	-2.709 (1.172)	10.876 (3.766)	**18.603**	None
	46.	5.031 (.302)			**18.723**	Measured Background[i]
	47.	5.654 (.843)	-2.576 (1.187)	10.866 (3.775)	**18.624**	Measured Background[i]
	48.	4.192 (.314)			**18.443**	Test Score
	49.	4.693 (.832)	[-2.283] (1.161)	10.058 (3.721)	**18.359**	Test Score
	50.	4.264 (.355)			**18.458**	Measured Background,[j] Age, Test Score
	51.	4.668 (.890)	[-2.176] (1.201)	10.455 (3.788)	**18.381**	Measured Background,[j] Age, Test Score
	52.	2.659 (.416)				Measured Background,[j] Age, Test Score, Early Occupation
	53.	4.098 (1.011)	-2.746 (1.351)	[6.166] (3.738)	**17.833**	Measured Background,[j] Age, Test Score, Early Occupation
	54.	4.002 (.524)			**17.836**	Family Background[k]
	55.	3.035 (1.426)	[-.092] (1.818)	13.700 (5.297)	**17.702**	Family Background[k]
	56.	3.499 (.557)			**17.702**	Family Background,[k] Test Score Difference
	57.	[2.389] (1.439)	[-.689] (1.807)	13.338 (5.260)	**17.570**	Family Background,[k] Test Score Difference

Table 6.2 Continued (5)

Sample	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
58.	2.150 (.639)			17.319	Family Background,[k] Test Score Difference, Early Occupation Difference
59.	[2.038] (1.418)	[-1.276] (1.784)	10.287 (5.241)	17.275	Family Background,[k] Test Score Difference, Early Occupation

a. Father's Education, Father's Occupation, Father White Collar, No Male Head, Nonfarm, NonSouth, Siblings, Father's Occupation by Race, Race, Siblings^2

b. Father's Education, Father's Occupation, Father White Collar, No Male Head, Nonfarm, NonSouth, Siblings, Father Foreign Born, Race

c. Father's Education, Nonfarm, NonSouth, Siblings, Father Foreign Born, Race

d. Father's Education, Father's Occupation, Father White Collar, No Male Head, Nonfarm, NonSouth, Race

e. Father's Education, Father's Occupation, No Male Head, Nonfarm, NonSouth, Race

f Father's Education, Father's Occupation, Non-farm, Siblings, Race

g Father's Education, Father's Occupation, Father White Collar, No Male Head, Nonfarm, Siblings, Race

k. Variables defined as sibling differences

h. **Father's Education, Father's Occupation, Siblings**

i. Father's Education, Father's Occupation, Siblings

j. Father's Education, Father's Occupation, Father White Collar, No Male Head, Siblings, Mother's Education, Father Foreign Born, Father Foreign coefficients are both proportionally and absolutely lower than those in the OCG and NLS.[8]

Family background is only imperfectly measured by socioeconomic variables. If the unmeasured aspects of family background which affect education are related to the unmeasured aspects of background which affect occupational status, controlling measured socioeconomic background will not suffice to eliminate bias due to background. By analyzing the relationships among sibling differences on education and occupation in our three samples of brothers, I have attempted to estimate the bias in the schooling-occupation relationship due to the effects of overall family background, and to indicate the extent to which this estimate of bias differs from estimates based solely on controlling measured background.[9] Unfortunately, the extent of bias introduced by measured background is substantially less in the brothers surveys than in our other samples. This may vitiate any generalizations about the relative importance of measured and unmeasured sources of bias. Evidence from the OCG suggests that this caution is warranted.

In the NORC Brothers sample, controlling measured background (and age) reduces the linear effect of an extra year of schooling on occupation from 4.634

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[8] The smaller reduction in the PA compared to the OCG is not due to the omission of a measure of father's occupation in the PA. Omitting Father's Occupation from the OCG background measures barely changes the estimated bias in the education coefficient.

[9] See Chapters 2 and 3 of the report as well as Olneck (1976), Corcoran, Jencks and Olneck (1976), and Taubman (1976) for further discussion.

points to 4.321 points, or by 0.313/4.634 = 6.8 percent. Controlling all aspects of family background that are common to brothers reduces the effect of education on occupation from 4.634 points to 3.161 points, or by 1.473/4.634 = 31.8 percent.

Among the Talent siblings, controlling measured background reduces the simple coefficient by only 0.017 points, or 0.017/7.324 = 0.2 percent. But in the regression of sibling occupational differences on educational differences, the reduction is 0.711 points, or 0.711/7.324 = 9.7 percent.

In the Kalamazoo Brothers sample, controlling measured background raises the estimated effect of education on occupation, albeit by an insignificant amount. But controlling common overall background reduces the effect by 1.010 points, or 1.010/5.012 = 20.2 percent.

The OCG survey did not ask respondents to report their brother's occupation but it did ask them their eldest brother's education. If brothers' characteristics do not directly affect one another and if respondents' reports about their brother's education are nearly as reliable as self-reports, we can use these data to calculate the within-pair effects of education on occupation.[10]

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[10] For the tenability of these assumptions see Olneck (1976), Chapter 4. I am grateful to Christopher Jencks for pointing out that these analyses could be conducted on the OCG data. Letting U denote respondent's education, U' denote brother's education, and Y denote respondent's occupation, the standardized coefficient (beta) with shared background control is $b = \frac{r_{YU} - r_{YU'}}{1-r_{UU'}}$. Assuming $S_U = S_{U'}$, the unstandardized coefficient is $B=(s_y)(r_{yu}-r_{yu'}) / (s_u)(1-{}_{uu'})$. For exposition of the model and equations underlying this result see Olneck (1976), page 160, and Chapter 2 of this report.

The simple correlation between education and occupational status among 5780 respondents in Jackson's OCG sample for whom brother's reports of educational attainment are available is 0.585. The within-pair standardized coefficient is 0.453. This suggests that the bias in the education-occupation relationships in the OCG that is due to shared background among brothers is $1 - 0.453/0.585 =$ 22.6 percent. This is only 4.3 percent more than the bias attributable to measured background in this same sample. The picture is somewhat different in OCG-II. In a sample of 6865 respondents aged 35-59 who reported a brother's education the correlation between education and occupation is 0.611. The standardized regression coefficient controlling father's education, father's occupation, siblings, male headed family, race, and farm background is 0.520. The within-family standardized coefficient is 0.469. Thus, controlling measured background suggests a bias in the schooling-occupation relationship of 15 percent, while controlling all background factors common to brothers suggests a bias of 23 percent.

I have not systematically examined all the possible reasons that the contributions of measured and unmeasured background factors to bias in the schooling coefficient differ between my 1962 and 1973 OCG samples. I did perform similar calculations by age cohorts on the published correlations in Duncan, Featherman, and Duncan [1972]. They suggest a small bias for men aged 25 to 34, but a very large bias for men 55 to 64 years of age. Therefore, the exclusion of men aged 25 to 34 from my OCG-II sample may be a factor in determining that difference. Reduced effects of race and farm background from 1962 to 1973 could also contribute to the difference. Note that the proportionate bias due to brothers' common background is virtually the same in both the 1962, and the 1973 samples, i.e., 23 percent. The absolute bias in the standardized coefficient is also quite similar across the two samples: 0.585 - 0.453 = 0.132 in the 1962 sample, and 0.611 - 0.469 = 0.142 in the 1972 sample. This suggests that the bias due to overall background is fairly constant, and insensitive to changes in the impact of measured background variables. However, this conclusion must remain tentative until other possible sources of difference in the results are examined. These include age composition, and also differences in the effects of measurement error across the two surveys.

Effects of Controlling Measured Ability

Measures of cognitive ability are related to educational attainment. They are also related to occupational status among men with equal amounts of schooling, though the extent to which this is true varies among our samples more than the strength of the schooling-test score relationship varies. Consequently, the estimate of the bias in the effect of schooling on occupational status that is due to the abilities measured by tests varies across samples.

Once education is controlled, the effect of test scores on occupation is trivial in the PSID [illegible]. The same is true for the Talent respondents. Consequently, the reduction in the education coefficient when test scores are controlled is smaller in these two samples than it is in the Veterans and Kalamazoo samples, where the continuing effects of test scores are stronger. Most of the Veterans respondents, however, took the AFQT after they had completed their schooling. If lengthier schooling improves cognitive skills, controlling AFQT scores in the Veterans sample will overestimate the bias in the schooling coeffi[illegible] that is due to prior ability. The coefficient of schooling, controlling AFQT, should therefore be interpreted simply as the effect of schooling among men with equal test scores, not as an estimate of the overall effect of schooling.

When test scores are controlled the bivariate coefficients fall by 0.246 points in the PSID, 0.412 among the Talent siblings, 0.685 for the NORC Veterans, and 0.820 for the Kalamazoo Brothers. Because the PSID test is not very reliable and because the Talent siblings are so few in number, I tend to put more faith in the Kalamozoo result as an estimate of the ability bias in the occupation-schooling relationship.[11/]

Effects of Controlling Both Ability and Family Background

Since background and test scores affect both schooling and occupation, we need to ask what the effects of schooling are among men who come from similar backgrounds and who also have similar initial ability. I can control only measured background and test scores after school completion in the Veterans and PSID samples. I can control all background factors which are common to brothers, as well as sibling test score differences, in Talent and Kalamazoo.

In the PSID, controlling only measured background reduces the effect of schooling on occupation from 3.910 to 3.579. The effect of education on occupation controlling both measured background and test scores is 3.438, or 3.438/3.910 = 87.9 percent of the uncontrolled effect. In the Veterans sample, the effect of a one year difference in education on occupational

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[11/] Jencks reports the reliability of the PSID test as only 0.652. See Mueser, App. D. Controlling test scores in the Wisconsin 1964 follow-up reduces the occupation-schooling coefficient from 8.501 to 7.755, or by 0.746 points.

status among men 30 to 34 who come from similar backgrounds and have similar test scores is 0.939/5.070 = 18.5 percent less than the bivariate coefficient.

Among the Talent siblings, the effect of a one year difference in schooling between brothers who have the same test scores is 6.506 points, or 6.506/7.324 = 88.8 percent of the uncontrolled effect. Among the Kalamazoo brothers the analogous results are 3.499 points and 3.499/5.012 = 69.8 percent of the uncontrolled effect.

Because the PSID test is questionable, because Veterans sample is selected partly on education and test scores, and because the Talent sibling sample is both selected on education and is small, I suspect that the estimate of bias in the occupation-schooling relationship due to background and cognitive ability in the Kalamazoo data is closest to the truth. However, scepticism about the results from a relatively small, locally restricted sample is certainly warranted.

Family background and cognitive ability do not exhaust the potential sources of bias in the schooling-occupation relationship. Men with more drive, perseverance, initiative, and other personality characteristics generally thought to promote career success may well get more schooling than those whose personalities are less attractive to employers.

. Brothers are certainly not fully alike on such characteristics, and so controlling family background will not adequately control their effects.

Our evidence on the bias imparted by the more favorable initial personality characteristics of the better-schooled and more successful is unfortunately weak. It comes from measures of personality characteristics rated by the homeroom teachers of the Kalamazoo respondents when they were in 10th grade.[12/]

Controlling these measures after IQ and measured background are controlled leaves the education coefficient virtually unchanged. This result may mean that personality ratings in 10th grade are a poor guide to adult characteristics, that these ratings are unreliable, that the specific characteristics teachers rate are not important to employers, or that the connection between personality characteristics and educational attainment is not as strong as employers who discriminate in favor of the better-educated think.

Effects of Controlling Early Occupation

The occupational advantage of better-educated men is due in part to their advantage in getting higher status jobs early in their careers and in part to being promoted more or engaging in more successful job changes than less-schooled men who begin their careers in occupations of similar status.

Controlling early occupational status among brothers in the Kalamazoo sample who have equal test scores reduces the effect of education by 3.499 - 2.150 = 1.349 points. The effect of education when background, test scores, and early occupation are controlled is 2.150/5.012 = 42.9 percent of the uncontrolled effect. This

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[12/] See Olneck (1976), Chapter 5. The attitudinal variable in the PSID were measured at the same time as the outcome measures, thereby introducing causal ambiguity. I have therefore ignored them in this section. I have ignored the Talent personality measures because at this writing no analysis of their effects on the education coefficients are available.

result suggests that employers reward credentials per se when they promote or hire workers with at least some experience, or that better educated men differ from less educated men in ways that escape our measurement. Possibly better educated men are favored in training and on-the-job learning opportunities.[13]

Differential Effects According to Level of Schooling

The preceding discussion does not distinguish the effects of different kinds of schooling. But completing high school does not lead to occupational advantages as large as those associated with completing college.[14] Moreover, the advantages associated with completing college are almost as large after controlling background and test scores as before. The advantages associated with completing high school are not only smaller to begin with but are reduced more when we control background and test scores.

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[13] This result may be particularly sensitive to measurement error. Measurement error corrections suggest that only 23 percent of the zero-order effect of education on occupation persists when family background, test scores, and initial occupation are controlled in the Kalamazoo data. See Olneck (1976), Chapter 4. However, for contrary results suggesting a small impact of measurement error on the education-occupation relationship net of early occupation in the OCG 1973 replication, see Bielby, Hauser, and Featherman (1976).

[14] I ignore the advantages associated with attending, but not completing college. This is because the meaning of our Years Post-Secondary Schooling and B.A. variables in ambiguous. If the effect of an extra year of graduate school is different from the effect of an extra year of college, the Years Post-Secondary variable will be misleading as a guide to the effect of attending but not completing college. In that case, the B.A. variable captures the departure of the slope for the college years from the slope estimated by Year Post-Secondary Schooling, as well as capturing strictly "diploma" effects.

In our four nationally representative samples, the predicted occupational advantage of a high school graduate over a grammar school graduate when background characteristics are controlled is from about 6 to 8 points, or 70 to 80 percent of the observed difference. The predicted advantage of college graduates over high school graduates with background characteristics controlled is close to 25 points, or 90 to 96 percent of the observed advantage in all four samples.

Our less representative samples also indicate that the effects of completing college are larger and more robust than the effects of completing high school. For example, in the Kalamazoo Brothers sample, controlling common family background and sibling test score differences reduces the advantages associated with completing four years of high school from 22.888 to 9.556 points, or 58.2 percent. For four years of college the reduction is from 22.928 to 20.138 points, or 12.2 percent. The proportionate reductions in the Veterans and NORC Brothers samples are similar.

We know that college graduates are not all equally bright. Indeed, our data show that the standard deviations of test scores for men with four years of college is from 70 to 85 percent of the overall standard deviations of test scores, depending on the sample. If high scores were important to performance in high status occupations, rational employers would presumably test applicants for jobs in such occupations and reject those with low scores. Since this does not happen, employers must believe that college graduates have valuable non-cognitive traits that high school graduates with similar test scores do not have.

This could either be because schools and colleges actually generate such traits, or it could be because they select on the basis of the characteristics employers value. If schooling generated the traits employers value in a simple additive way, I would expect each year of education to confer similar benefits. Since a year of high school confers less benefit than a year of college, and since college graduates have a greater advantage over college dropouts than predicted using linear models, I suspect that education does not generate economically desirable characteristics in such a simple manner. Either colleges are more effective than high schools in augmenting students' productive capacities, or else colleges sort and certify students more effectively than high schools, using pre-existing characteristics not measured in our surveys. Another alternative is that employers and legislators who set requirements for entering high status occupations favor college graduates for reasons that have nothing to do with their personal characteristics.

Age Differences in the Occupational Effects of Education

Men who differ in age differ both in cohort membership and in the point at which they stand in the lifecycle. Consequently, observed differences in the effects of education across age groups may be due to historical trends, age differences, or both.

The 1973 OCG (i.e. OCG-II) suggests, however, that the effects of educational attainment on occupational status are stable for most of an individual's career.[15] Census reports on respondents' occupation in 1965 and 1970 support this conclusion.[16] Both the Kalamazoo and OCG-II data suggest, however, that education has more impact on first occupation than on later occupations.[17] I have interpreted the intercohort comparisons in the effects of education in our data as measurements of the historical trend in the relationship between schooling and occupational status (See Table 6.3).

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[15] Within-cohort education coefficients, controlling measured background, show no significant differences between 1962 and 1973. See Featherman and Hauser (1975)

[16] Unpublished tabulations by Bartlett and Jencks.

[17] Olneck, Appendix I, Tables 14A2 vs 14A4, and Bielby et al (1976).

The most reliable evidence we have for intercohort differences in the occupational effects of education comes from the 1970 Census and the 1962 OCG study. The numbers of respondents in individual cohorts in our other samples are too small to allow meaningful comparisons. The 1970 Census data suggest that the effect of an extra year of schooling below the college level is slightly larger among men 35 and over than among 25-34 men, though only the coefficient for 30 to 34 year olds differs significantly from the coefficients for older cohorts. Moreover, the OCG equations which control measured background show no significant intercohort differences in the effects of elementary and secondary schooling. Since the effects of some measured background variables on education declined from 1962 to 1973,[18/] I would expect controlling measured background to reduce the schooling coefficient on occupation more for older men than for younger men. This would make the 1970 Census results consistent with the 1962 OCG.[19/]

The OCG data suggest that the occupational benefits of higher education are much greater for younger men. With measured background controlled four years of college boost expected occupational status by 32 points for 25-34 year olds, 27 points for 35-44 year olds, 22 points for 45-54 year olds, and 15 points for 55-64 year olds.

[18/] See Hauser and Featherman (1976)

[19/] The 1973 OCG data do suggest that the linear effect of education on occupation is systematically higher for younger individuals, but this result probably reflects nonlinearities in the effects of education combined with rising mean attainment.

Table 6.3 Effects of Education on Current Occupational Stratified by Age

Sample		Years of Education	Years of Post Secondary Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1970 Census						
25-29 (N=3748)	1.	2.722 (.318)	3.621 (.555)	4.700 (1.786)	18.248	Experience, Experience^2
30-34 (N=3375)	2.	2.285 (.284)	3.797 (.564)	5.563 (1.996)	18.678	Same as Equation 1
35-44 (N=6963)	3.	3.061 (.136)	2.213 (.332)	4.184 (1.423)	18.864	Same as Equation 1
45-54 (N=6834)	4.	3.082 (.127)	2.091 (.354)	3.989 (1.566)	19.136	Same as Equation 1
55-64 (N=4777)	5.	3.129 (.132)	2.895 (.457)	[1.072] (2.092)	18.503	Same as Equation 1
OCG 25-34 (N=3166)	6.	2.385 (.191)	3.478 (.485)	8.246 (1.970)	17.586	Measured Background, Experience
35-44 (N=3443)	7.	2.366 (.165)	2.279 (.503)	8.063 (2.194)	18.302	Same as Equation 6
45-54 (N=2951)	8.	2.285 (.174)	2.344 (.580)	[3.267] (2.647)	18.414	Same as Equation 6
55-64 (N=1944)	9.	2.208 (.212)	3.453 (.853)	[-7.294] (3.845)	19.328	Same as Equation 6

Table 16.3 Continued (2)

Sample		Years of Education	Years of Post Secondary Education	BA	Standard Deviation of Residuals	Other Variables Controlled
PSID						
25-34 (N=545)	10.	2.561 (.656)	2.557 (1.177)	[4.161] (3.682)	16.090	Measured Background,[b] **Vocational** Training, Experience
35-44 (N=528)	11.	1.832 (.489)	2.518 (1.085)	10.655 (4.147)	15.813	Same as Equation 10
45-54 (N=431)	12.	1.519 (.440)	4.275 (1.125)	[2.205] (4.436)	14.766	Same as Equation 10
55-64 (N=270)	13.	[.751] (.537)	[2.909] (1.582)	[6.494] (6.661)	15.964	Same as Equation 10
Productive Americans						
25-34 (N=290)	14.	1.560 (.663)	6.790 (1.612)	[-1.492] (5.544)	15.161	Measured Background,[c] Vocational Training, Experience
35-44 (N=338)	15.	1.572 (.555)	3.777 (1.508)	[-1.141] (5.375)	15.768	Same as Equation 14
45-54 (N=331)	16.	1.278 (.508)	3.869 (1.631)	[.716] (6.368)	16.093	Same as Equation 14
55-64 (N=229)	17.	2.132 (.553)	6.363 (2.018)	[-7.414] (8.554)	15.416	Same as Equation 14
Kalamazoo Brothers						
35-44 (N=279)	18.	5.589 (1.414)	[-2.561] (1.805)	[4.957] (5.513)	17.906	Measured Background,[d] Test Score
45-54 **(N-413)**	19.	4.403 (1.145)	[-2.355] (1.612)	14.571 (5.119)	18.699	Same as Equation 13

a. Father's Education, Father's Occupation, Father White Collar, No Male Head, Nonfarm, NonSouth, Siblings, Race

b. Father's Education, Father's Occupation, Father White Collar, No Male Head, Nonfarm, NonSouth, Siblings, Race

c. Father's Education, Nonfarm, NonSouth, Siblings, Race

d. Father's Education, Father's Occupation, No Male Head, Siblings

There is no reason to suppose that controlling unmeasured background or ability would appreciably alter the foregoing picture. I conclude that the occupational benefits of elementary and secondary education have been modest and stable. The benefits of higher education appear to have been somewhat higher than for elementary and secondary education for many years, but they also appear to have risen sharply for recent cohorts, at least up to 1962.

Racial Differences in the Occupational Effects of Education

It is commonly thought that the credentials held by nonwhites and whites are rewarded unequally. Our evidence suggests that while nonwhites have lower occupational statuses than whites with the same amount of schooling, the increase in status associated with college attendance is at least as large and possibly larger for non-whites than for whites (See Table 4.4). In all four of our data sets with substantial numbers of nonwhites, the predicted status advantage of a nonwhite college graduate over a nonwhite high school graduate is larger than the predicted advantage of a white college graduate over a white high school graduate. Rather than indicating any special advantage enjoyed by nonwhite college graduates, this probably reflects the dismal treatment accorded nonwhites without college degrees.[20]

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[20] For additional regressions see the appendix of Chapter 8.

Table 6..4 Effects of Education on Current Occupational Status Stratified by Race

Sample		Years of Education	Years of Post Secondary Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1970 Census						
White (N=23615)	1.	3.217 (.065)	2.211 (.173)	4.165 (.790)	18.833	Experience, Experience^2
Non-White (N=2082)	2.	1.481 (.134)	5.015 (.560)	[2.891] (2.910)	15.721	Same as Equation 1
OCG						
White (N=10395)	3.	2.708 (.094)	2.365 (.299)	5.221 (1.283)	18.715	Measured Background,[a] Experience, Experience^2
Non-white (N=1110)	4.	.804 (.152)	3.509 (.866)	21.103 (4.012)	13.844	Same as Equation 1
PSID						
White (N=1260)	5.	1.476 (.297)	3.379 (.703)	5.129 (2.552)	15.938	Measured Background,[b] Test Score, Vocational Training, Experience Experience^2
Non-white (N=514)	6.	1.473 (.273)	[1.116] (1.085)	25.166 (5.298)	13.081	Same as Equation 4
NLS ; 45-59 years olds						
White (N=2580)	7.	2.043 (.194)	3.112 (.643)	5.413 (2.747)	18.672	Measured Background,[c] Vocational Training, Experience, Experience^2
Non-white (N=250)	8.	.671 (.322)	7.219 (2.256]	[-.857] (10.921)	13.249	Same as Equation 5

a,b. Father's Education, Father's Occupation, Father White Collar, No Male Head, Nonfarm, NonSouth, Siblings

c. Same as a and b, except does not include Siblings

Our evidence concerning the occupational benefits of elementary and secondary education is more consistent with the conventional wisdom concerning racial differences in the effects of schooling. In three of the four data sets , the effect of an extra year of schooling below the college level is significantly higher for whites than nonwhites. In the PSID, the effects are virtually identical for whites and nonwhites. This is in part because controlling background and test scores reduces the coefficient of years of education more for whites than nonwhites. It may also be because nonwhite heads of households are less representative of nonwhites in general than white heads of households are of whites in general.

If these results are correct, they suggest that nonwhites who pursue a college education will realize substantial occupational benefit, but that those who quit high school before graduating will not suffer a substantial loss in occupational status relative to those who complete high school but go no further. From the point of view of policies pertaining to school retention, however, these results should be viewed cautiously unless they are substantiated with data on current youths.

Ability Differences in the Occupational Effects of Education

One plausible reason why schooling might affect occupational status even with test scores controlled is that schools teach cognitive skills or knowledg not measured by our tests. This seems especially plausible with respect to higher education, which seldom stresses the basic skills most tests try to measure. But if this theory were correct, I would expect individuals with hig initial test scores to realize larger occupational benefits from any given amount of schooling than individuals with low initial scores. This is because more individuals with good basic skills can presumably learn more in a given amount of time than individuals with poor basic skills.

Table 6.5 displays the effects of education . . for separate ability groups. There is no good evidence that the occupational benefits of extra schooling are larger for men with high test scores than for men with low test scores. Nor were the education-test score interactions significant in any of the samples with such scores. Either low scoring individuals acquire economically relevant skills and knowledge from schooling as quickly as high scoring individuals, or else neither group acquires such skills and employers value credentials for other reasons.

Differences in the Occupational Effects of Education by Father's Occupational Group

More and better schooling is frequently proposed as a way to increase the economic life chances of poor children. It is therefore important to ask whether the occupational benefits of schooling are as great for men from disadvantaged backgrounds as for the population in general. Unfortunately, none of our data sets includes direct information on parental income. As a partial substitute, we have stratified our samples according to whether a respondent's father held a white-collar, blue-collar, or farm job. This should give us some indication of whether the effects of schooling are similar for men from disadvantaged and advantaged homes.

Table 6.5 Effects of Education on Current Occupational Status Stratified by Test Score (Q).

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
PSID Q = 1-9[a] (N=764)	1.	1.589 (.270)	2.284 (1.044)	12.245 (4.607)	14.716	Measured Background,[b] Test Score, Vocationa Training, Experience, Experience^2
Q = 10-11 (N=707)	2.	1.489 (.427)	3.050 (956)	8.482 (3.365)	16.240	Same as Equation 2
Q = 12-13 (N=303)	3.	1.744 (.830)	[2.664] (1.402)	[2.699] (4.396)	15.534	Same as Equation 2
Veterans						
Q below 31st percentile[c] (N=236)	4.	[.557] (.612)	5.003 (2.219)	[18.451] (11.158)	16.504	Measured Background,[d] Test Score
Q from 31st to 64th percentile (N=264)	5.	[.762] (.892)	5.845 (1.648)	[3.914] (6.396)	15.882	Same as Equation 5
Q Above 64th percentile (N=303)	6.	[3.569] (1.868)	[.690] (2.357)	[6.467] (4.830)	19.059	Measured Background,[d] Test Score

Table 6.5 Continued (2)

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
Talent						
Q Below 90[e] (N=173)	7.	5.698 (1.453)			**17.212**	Measured Background,[f] Test Score, Education, Experience
Q = 90 - 110 (N=395)	8.	5.075 (.602)			**18.777**	Same as Equation 7
Q = Over 110 (N=271)	9.	5.220 (.708)			**16.677**	Same as Equation 7
Kalamazoo Brothers						
Q Below 90 (N=168)[g]	10.	4.003 (1.401)	[3.057] (3.294)	[-6.157] (13.523)	**19.364**	Measured Background,[h] Test Score
Q = 90 to 110 (N=349)	11.	5.749 (1.482)	[-3.269] (2.003)	10.440 (5.854)	**19.306**	Measured Background,[h] Test Score
Q = Over 110 (N=175)	12.	[-.803] (3.696)	[-2.710] (3.913)	13.011 (4.659)	**15.274**	Measured Background,[h] Test Score

a. 13 item Sentence Completion Test ($\bar{Q} = 10.0$, $s_q = 2.0$)
b. Father's Education, Father's Occupation, Father White Collar, No Male Head, Nonfarm, NonSouth, Siblings, Race

c. AFQT, Scaled to national norms ($\bar{Q} = 103.4$, $s_q = 13.7$)
d. Father's Education, Father's Occupation, No Male Head, Nonfarm, NonSouth, Race

e. Academic Achievement Composite, scaled to national norms. ($\bar{Q} = 102.6$, $s_q = 14.6$)
f. Father's Education, Father's Occupation, No Male Head, Siblings, Race

g. Terman or adjusted Otis IQ ($Q = 100.9$, $s_q = 15.3$)
h. Father's Education, Father's Occupation, No Male Head, Siblings

All samples in Table 6.6 show greater occupational benefits from pre-college education for men with white-collar fathers than for men with blue-collar fathers, but the differences are not statistically significant in any of our samples. In the OCG and Parnes data, the effects of elementary and secondary schooling are significantly lower for farm-born respondents than for others, but the difference is not significant for Veterans and is not even present in PSID. It is possible that the Parnes sample, which covers men 45 to 59 years old in 1966, and the OCG, which was conducted in 1962, include larger proportions of high school graduates from farm backgrounds who remained in farming than do the Veterans and PSID samples. If this were the case, high school graduation would confer smaller occupational benefits for men with farm backgrounds than for others.

Our evidence is mixed with respect to the occupational advantages gained from going to college by respondents with white-collar and blue-collar fathers. No consistently significant pattern is evident, and few of the individual coefficients are significantly different. On the other hand, men from farm backgrounds gain significantly more by graduating from college than do others. Farm sons are similar in this respect to non-whites.

Table 6.6 Effects of Education on Current Occupational Status Stratified by Father's Occupational Group

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1962 OCG						
Father White Collar (N=2631)	1.	2.879 (.317)	1.635 (.571)	3.729 (1.871)	19.004	Measured Background,[a] Experience, Experience
Father Blue Collar (N=4915)	2.	2.604 (.136)	2.221 (.466)	10.991 (2.094)	18.647	Same as Equation 1
Father Farm (N=3288)	3.	1.943 (.128)	3.168 (.647)	10.185 (3.089)	17.197	Same as Equation 1
PSID						
Father White Collar (N=329)	4.	2.966 (.910)	[.397] (1.403)	4.776 (3.811)	14.740	Measured Background,[b] Vocational Training, Test Score, Experience Experience^2
Father Blue Collar (N=862)	5.	1.248 (.339)	3.832 (.878)	[6.573] (3.422)	15.947	Same as Equation 4
Father Farm (N=583)	6.	1.285 (.339)	4.446 (1.089)	9.090 (4.484)	15.494	Same as Equation 4
NLS 45-59 years old						
White Collar (N=550)	7.	3.290 (.592)	1.963 (1.183)	[6.417] (4.232)	18.299	Measured Background,[c] Vocational Training, Experience
Blue Collar (N=1438)	8.	2.232 (.246)	4.204 (.893)	[-1.984] (4.179)	18.942	Same as Equation 7
Farm (N-825)	9.	.965 (.268)	[1.334] (1.307)	24.201 (6.372)	16.756	Same as Equation 7
Veterans 30-34 year olds						
White Collar (N=153)	10.	[2.169] (1.549)	2.506 (2.184)	7.474 (5.853)	17.147	Measured Background,[d] Test Score, Test Score, by Education
Blue Collar (N=415)	11.	[1.244] (.671)	5.281 (1.320)	[.246] (5.164)	17.031	Same as Equation 10
Farm (N=143)	12.	[-.615] (.930)	[3.029] (2.628)	28.173 (11.999)	18.042	Same as Equation 10

Table 6.6 Continued (2)

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
Talent 28 year olds						
White Collar (N=315)	13.	4.532 (.700)			17.917	Measured Background,[e] Education,^{2} Experienc
Blue Collar (N=448)	14.	5.103 (.557)			17.982	Same as Equation 13
Kalamazoo Brothers						
White Collar (N=278 individuals or 139 pairs)	15.	4.4118 (1.817)	[-2.631] (2.107)	12.026 (4.591)	16.890	Measured Background,[f] Test Score
	16.	[3.151] (2.807)	[-3.054] (3.225)	20.126 (7.044)	16.792	Family Background,[g] Test Score Difference
Blue Collar (N=414 individuals or 207 pairs)	17.	4.490 (1.035)	[-1.086] (1.605)	[8.581] (6.000)	19.422	Measured Background,[f] Test Score
	18.	[2.208] (1.715)	[.966] (2.293)	[5.997] (7.845)	18.170	Family Background,[g] Test Score Difference

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a. Father's Education, Father's Occupation, No Male Head, NonSouth, Siblings, Race

b. Father's Education, Father's Occupation, No Male Head, NonSouth, Siblings, Race

c. Father's Education, Father's Occupation, No Male Head, NonSouth, Race

d. Father's Education, Father's Occupation, No Male Head, NonSouth, Race

e. Father's Education, Father's Occupation, No Male Head, NonSouth, Race

f. Father's Education, Father's Occupation, Siblings

Section 4. Effects of Education on Earnings and Income

Occupational status is an important dimension on which our society is stratified. The available evidence suggests, however, that most people put greater weight on income than on status (see Chapter 11). Certainly economists do. While occupational status is correlated with income, the correlation seldom exceeds 0.50. And the factors affecting income are often quite different from those affecting occupational status. This section therefore looks at the effects of schooling on income and earnings.

Because income has risen over time and because of sampling differences, the distributions of income are not the same across our data sets. However, if changes in the effects of education are proportional for all groups, a log transformation of income will yield similar results across samples from different years. I have therefore used the natural logarithm of earnings or income as the dependent variable in my analyses. Sometimes I will speak of the effects of education in log dollars. This convention refers to the observed coefficients. Sometimes I will speak of the effects of education in terms of percentage changes. This convention refers to the anti-logs of the observed coefficients minus 1.00. (The two are very close when the observed coefficient is small.)

The regression results shown in Table 6.7 include both equations that control experience and equations that do not. If men who get more schooling work as many years as men who get less schooling, ignoring experience will bias downwards the estimates of education averaged over a working life. However, if men with more schooling work fewer years than men who quit school earlier, the lifetime effects of schooling are best estimated with experience excluded. Mincer (1974) reports that the average length of a working life for men with 8, 9-11, and 12 or fewer years of schooling averages 47 years. For men with 13-15, 16, and 17 or more years of schooling, working life averages 45 years. This implies that an extra year of elementary or secondary schooling is generally accompanied by an extra year of work, that men who begin college do not extend their working lives to compensate completely for their first years of college, but that men who remain in college do extend their working lives to make up for later years of higher education. These estimates appear to ignore the fact that highly educated men live longer than poorly educated men (Kitagawa and Hauser, 1973). Taking this into account, Jencks concludes in Chapter 14 that extra education does not appreciably shorten men's working lives. In what follows I will therefore discuss equations with experience controlled.

Table 6.7 Effects of Education on Natural Logarithm of Earnings (or Income for OCG)

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1970 Census (N=25,697)	1.	.0785 (.0012)			.661	None
	2.	.0818 (.0019)	-.0255 (.0058)	.1110 (.0270)	.661	None
	3.	.0867 (.0013)			.650	Experience, Experience^2
	4.	.0849 (.0020)	-.0166 (.0057)	.1256 (.0266)	.650	Experience, Experience^2
1962						
(N=11,504)	5.	.0898 (.0019)			.749	None
	6.	.1057 (.0029)	-.0924 (.0113)	.2743 (.0498)	.747	None
	7.	.1005 (.0021)			.741	Experience, Experience^2
	8.	.1128 (.0031)	-.0837 (.0112)	.2857 (.0493)	.740	Experience, Experience^2
	9.	.0656 (.0022)			.721	Measured Background[a]
	10.	.0778 (.0030)	-.0822 (.0110)	.2716 (.0480)	.720	Measured Background[a]
	11.	.0732 (.0024)			.714	Measured Background[a] Experience, Experience^2
	12.	.0814 (.0032)	-.07212 (.0109)	.2840 (.0475)	.713	Measured Background[a] Experience, Experience^2
1971 PSID (N=1744)	13.	.1001 (.0048)			.675	None

Table 16.7 Continued (2)

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
	14.	.1042 (.0082)	[-.0494] (.0237)	.2314 (.0930)	.675	None
	15.	.0931 (.0053)			.655	Experience, Experience ^{2}
	16.	.0836 (.0087)	[-.0110] (.0235)	.1765 (.0909)	.654	Experience, Experience ^{2}
	17.	.0874 (.0056)			.666	Measured Background[b]
	18.	.0868 (.0088)	[-.0444] (.0241)	.2517 (.0930)	.664	Measured Background[b]
	19.	.0804 (.0054)			.664	Test Score
	20.	.0813 (.0086)	[-.0441] (.0235	.2389 (.0921)	.663	Test Score
	21.	.0747 (.0059)			.658	Measured Background,[b] Test Score
	22.	.0726 (.0090)	[-.0419] (.0238)	.2556 (.0920)	.657	Measured Background Test Score
	23.	.0654 (.0062)			.637	Measured Background, Test Score, Experience, Experience ^{2}
	24.	.0512 (.0093)	[-.0086] .0233	.2113 (.0391	.636	Measured Background, Test Score, Experience, Experience ^{2}
1964 Productive Americans (N=1188)	25.	.0995 (.005)			.618	None
	26.	.1036 (.008)	[-.0171] (.029)	[.0295] (.118)	.618	None
	27.	.1080 (.0059)			.615	Experience
	28.	.1136 (.0085)	[-.0229] (.0293)	[.0419] (.1176)	.616	Experience

Table 6.7 Continued (3)

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residual	Other Variables Controlled
	29.	.0782 (.0059)			.595	Measured Background^c
	30.	.0783 (.0083)	[-.0099] (.0289)	[.0513] (.1145)	.596	Measured Background^c
	31.	.0849 (.0066)			.594	Measured Background^c, Experience
	32.	.0862 (.0090)	[-.0152] (.0290)	[.617] (.1144)	.595	Measured Background^c, Experience
1965 NLS 45-59 year olds (N=2830)	33.	.1051 (.0041)			.794	None
	34.	.1069 (.0059)	[-.0198] (.0256)	[.0814] (.1145)	.794	None
	35.	.0824 (.0048)			.777	Measured Background^d
	36.	.0792 (.0065)	[.0010] (.0253)	[.0525] (.1127)	.777	Measured Background^d
	37.	.0686 (.0058)			.774	Measured Background^d Experience
	38.	.0665 (.0072)	[-.0027] (.0252)	[.0564] (.1124)	.775	Measured Background^d, Experience
1964 Veterans 30-34 year olds (N=803)	39.	.0565 (.0061)			.473	None

Table 6.7 Continued (4)

Sample	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
40.	.0532 (.0116)	[-.0012] (.0246)	[.0433] (.0945)	.473	None
41.	.0964 (.0140)			.470	Experience
42.	.0952 (.0177)	[-.0055] (.0245)	.0466 (.0940)	.471	Experience
43.	.0425 (.0064)			.459	Measured Background^e
44.	.0381 (.0115)	[-.005] (.0239)	[.0500] (.0911)	.455	Measured Background^e
45.	.0308 (.0071)			.461	Test Score
46.	.0244 (.0121)	[.0000] (.0240)	[.0661] (.0921)	.461	Test Score
47.	.0244 (.0073)			.448	Measured Background^e, Test Score
48.	[.0181] (.0120)	[-.006] (.0236)	[.0690] (.0898)	.449	Measured Background^e, Test Score
49.	.0557 (.0143)			.447	Measured Background^e, Test Score, Experience
50.	.0509 (.0179)	[-.0045] (.0236)	[.0714] (.0895)	.447	Measured Background^e, Test Score, Experience

Table 6.7 Continued (5)

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1974 NORC Brothers (N=300 individuals or 150 pairs)	51.	.0997 (.0152)			.814	None
	52.	.1506 (.0286)	[-.1110] (.0645)	[.1375] (.2834)	.810	None
	53.	.09632 (.01722)			.820	Measured Background[f] Age
	54.	.157 (.032)	[-.124] (.067)	[.184] (.294)	.810	Same as Equation 54
	55.	.1097 (.0211)			.778	Family Background[g],
	56.	.156 (.048)	[-.109] (.092)	[-.085] (.394)	.774	Same as Equation 53
1972 Talent 28 year olds (N=839)	57.	.0364 (.0055)			.387	None
	58.	.0567 (.0077)			.384	Experience
	59.	.0299 (.0061)			.386	Measured Background[h]

Table 6.7 Continued (6)

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
	60.	.0260 (.0066)			.385	Test Score
	61.	.0221 (.0069)			.385	Measured Background,[h] Test Score
	62.	.0429 (.0085)			.381	Measured Background,[h] Test Score, Experience
1971-72 Talent Brothers (N=198 individuals or 99 pairs)	63.	.0604 (.0110)			.380	None
	64.	.0707 (.0124)			.376	Measured Background[i]
	65.	.0388 (.0140)			.375	Test Score
	66.	.0494 (.0146)			.370	Measured Background[i] Test Score
	67.	.0566 (.0214)			.352	Family Background[g]
	68.	[.0420] (.0233)			.349	Family Background[g] Test Score, Difference

Table 6.7 Continued (7)

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1974 **Kalamazoo Brothers** (N=692 individuals or 346 pairs)						
	69.	.0671 (.0057)			.411	None
	70.	.0792 (.0177)	[-.0265] (.0257)	[.0645] (.0825)	.407	None
	71.	.0642 (.0066)			.412	Measured Background[j]
	72.	.0742 (.0185)	[-.0224] (.0260)	[.0582] (.0826)	.408	Measured Background[j]
	73.	.0492 (.0069)			.406	Test Score
	74.	.0558 (.0182)	[-.0167] (.0254)	[.0459 (.0814)	.402	Test Score
	75.	.0480 (.0075)				Measured Background,[j] Test Score
	76.	.0535 (.0188)	[-.0144] (.0257)	[.0413] (.0816)	.402	Measured Background[j] Test Score
	77.	.0499 (.0113)			.384	Family Background[g]
	78.	[.0474] (.0310)	[-.0237] (.0395)	[.1772] (.1150)	.384	Family Background[g]
	79.	.0310 (.0118)			.374	Family Background[g] Test Score Differenc
	80.	[.0229] (.0306)	[-.0148] (.0385)	[.1635] (.1120)	.374	Family Background[g], Test Score Difference

Table 6.7 Continued (8)

- a. Father's Education, Father's Occupation, Father White Collar, No Male Head, Nonfarm, NonSouth, Siblings, Siblings,^{2} Race, Father's Occupation by Race
- b. Father's Education, Father's Occupation, Father White Collar, Father Foreign Born, No Male Head, Nonfarm, NonSouth, Siblings, Race
- c. Father's Education, Father Foreign Born, Nonfarm, NonSouth, Siblings, Race
- d. Father's Education, Father's Occupation, Father White Collar, No Male Head, Nonfarm, NonSouth, Race
- e. Father's Education, Father's Occupation, No Male Head, Nonfarm, NonSouth, Race
- f. Father's Education, Father's Occupation, Father White Collar, No Male Head, Nonfarm, Siblings, Race
- g. Variables defined as sibling differences
- h. Father's Education, Father's Occupation, Siblings, No Male Head, Race
- i. Father's Education, Father's Occupation, Siblings
- j. Father's Education, Father's Occupation, Siblings

In young samples (e.g. Talent, Veterans), including experience implies smaller proportionate biases due to test scores and socioeconomic background. In older samples, the proportionate bias attributable to ability and background is somewhat, though not substantially, larger when experience is included.

The Census, PSID, and PA surveys suggest that an extra year of schooling is associated with an 8 to 11 percent increment in annual earnings for men 25 to 64 with the same amount of experience. Lower reliability of the education measure in the Census and coding and sample peculiarities in the PSID and PA probably account for these differences.[21/] The bivariate effect of schooling on earnings is probably close to 10 percent

The OCG study measured annual income rather than earnings. It suggests that an extra year of schooling is associated with an 11 percent increase in annual income for men with equal experience. McClelland's work with the PSID indicates that substituting income for earnings does not greatly change the estimate of the bivariate effect of schooling, so I will discuss results from the OCG at the same time that I discuss results from the other surveys, and will not distinguish earnings from income.

[21/] See McClelland, Chapter 15.

Effects of Controlling Family Background

In the OCG, PSID, and PA surveys, an additional year of schooling is associated with a 7.6 to 8.9 percent increase in earnings among men from similar demographic backgrounds and experience. This means that the observed relationships between schooling and Ln Earnings overestimate the actual effects because men from favored background enjoy earnings advantages that are independent of their educational attainments. Our results suggest that for men 25 to 64 with equal experience, 20 to 25 percent of the apparent relationship between schooling and earnings arises for this reason. The Parnes data suggest a similar bias for men 45 to 59 years old.

It is possible that unmeasured aspects of family background impart biases to the income-schooling relationship which are not removed when only demographic background is controlled. In the NORC Brothers survey, however, the regression coefficient for schooling differences between brothers with age differences controlled is only trivially different from the coefficient when demographic background and age differences among individuals are controlled (0.09439 vs. 0.09632). Moreover, when age differences are ignored, the within-pair coefficient is slightly higher than the simple bivariate coefficient (0.10972 vs. 0.0997).

In the Talent sibling sample, controlling measured background raises the schooling coefficient by 0.0104. Controlling family background common to brothers reduces it, but by only 0.0038. The NORC Brothers and Talent siblings data therefore suggest that unmeasured family background is a minor source of bias in the schooling-income relationship.

The Kalamazoo Brothers data suggest the opposite conclusion. The regression coefficient of sibling differences in Ln Earnings on differences in Years of Education is 0.0499. That is 0.0172 or 0.0172/0.0671 = 25.6 percent less than the simple bivariate coefficient. It is 0.0143 less than the coefficient when measured background is controlled. Because the Kalamazoo sample is considerably larger than our other two brothers sample, the Kalamazoo estimate has a smaller sampling error. Unfortunately, the effects of measured background on both occupation and income are substantially lower in the Kalamazoo sample than in nationally representative samples. This makes the generality of the Kalamazoo findings suspect.

Controlling measured background in Jackson's OCG complete data sample reduces the bivariate coefficient of schooling for Ln Income from 0.0898 to 0.0656, or by 0.0242/0.0898 = 27 percent. Among 5780 OCG respondents who reported their eldest brother's education, the correlation between Ln Income and education is 0.385. The within-pair standardized coefficient is 0.273, which suggests a bias due to sibling's common background of[((0.385 - 0.273)/0.385] = 29.1 percent.[22/]

[22/] See footnote 10 for derivation. The within-pair coefficient is

$$\frac{r_{UY} - r_{U'Y}}{1 - r_{UU'}} = \frac{0.385 - 0.277}{1 - 0.605} = 0.273$$

where U = respondent's education, U' = respondent's report of his brother's education, and Y = respondent's income.

This result suggests that the family background factors that affect both education and earnings, like those that affect both education and occupation, are largely captured by OCG's demographic measures. Again, however, evidence from the 1973 OCG replication suggests otherwise. Among the 6865 respondents aged 35 to 59 and reporting their brothers' educations, the correlation between Education and Ln Income is 0.396 With measured background controlled, the standardized coefficient of education is 0.318. Controlling brothers' common background, the standardized coefficient is only 0.252. The bias in the income-schooling relationship due to background appears on the order of 36 percent, rather than the 20 percent suggested by controlling only measured socioeconomic variables. The results for Ln Income are similar to those for income, but not as dramatic.

Effects of Controlling Cognitive Ability

Economists have devoted considerable attention to the possibility that the apparent returns to schooling are inflated by a correlation between educational attainment and initial ability. Unless one is willing to define ability, of course, empirical research can never resolve this issue. Cognitive tests measure only a subset of abilities. Getting through school and succeeding at work may require many abilities which are not measured by such tests. Nonetheless, controlling the test scores available in our data sets close to an upper limit on the true returns to schooling.23/

23/ Measurement error will reduce the coefficient from its true value somewhat. The extent to which this is true is probably small. See pp. 64-47 below.

Our efforts to measure the ability bias are limited by the fact that the Veterans and PSID tests were administered to respondents after most of them had completed their schooling. If increased schooling raises test scores, we will overestimate the biases due to ability in those samples. We can only estimate the effects of schooling that are independent of test scores, not the unbiased (or less biased) effects of schooling in those data sets.

The effects of schooling on Ln Earnings are significantly attenuated among men with the same test scores. Controlling adult test scores (but not background or experience) reduces the coefficient of education by 0.0197 in the PSID and 0.0257 in the Veterans sample - reductions of 20 and 46 percent respectively. Controlling adolescent test scores reduces the coefficient of education by 0.0216 among Talent brothers and 0.0179 among the Kalamazoo brothers - reductions 36 and 27 percent respectively.

Cumulative Reductions in the Effects of Education Due to Background and Ability

The effects of schooling are even lower when men have the same test scores and also come from similar backgrounds. The coefficient of schooling controlling both measured background and adult test scores is 0.0747 instead of 0.1001 in PSID and 0.0244 instead of 0.0565 in the Veterans sample. Controlling demographic background and adolescent test scores reduces the Talent coefficient from 0.0567 to 0.0429 (with experience controlled in both cases). Controlling brothers' common background and sibling test score differences reduces the uncontrolled effect by from 0.0671 to 0.0310 in the Kalamazoo Brothers sample. Because the correlation between test scores and schooling is unusually low in the Michigan data, and because the Talent brothers are young and are almost all at least high school graduates, and the effects of post-secondary schooling are more robust than those of elementary and secondary schooling, I place greater faith in the generality of results from the Veterans and Kalamazoo samples than from the Michigan and Talent samples.[24] They suggest that at least half the observed effect of schooling on ln Earnings disappears when family background and cognitive ability are controlled. With experience controlled in the Veterans sample, the implied bias (net of experience) is 42 percent. The absolute bias is between 0.03 and 0.04. It is not clear whether one should expect the absolute or relative bias to be more generalizable.

It is likely that if we could take into account additional differences between men with more and less schooling, we would find that the effects of schooling on income would be further reduced. Among 389 respondents in the Kalamazoo sample for whom measured background, test score, teacher personality ratings, and follow-up data are available, adding a rating of Executive Ability in 10th grade to an earnings equation already including socioeconomic background and test scores, reduces the effect of education by an additional 97 dollars, or by 97/1119 = 8.7 percent of the effect with only background and test scores controlled (Olneck, 1976). Unfortunately, our data are inadequate for extensive exploration of the biasing effects of noncognitive characteristics.

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[24] The correlation between test scores and education is only 0.473 in the PSID. It is 0.554 in the Veterans sample, 0.606 among the Talent siblings, and 0.576 for the Kalamazoo brothers.

Differential Effects According to Levels of Schooling

The average year of higher education is associated with a smaller percentage increase in earnings than the average year of education below the college level. But graduating from college confers substantial economic benefits, so that in most of our data sets the percentage increase in earnings associated with four years of college is at least as great as that associated with four years of high school. Only in the PSID does the benefit associated with four years of college differ by more than 10 percentage points from the benefits associated with four years of high school. Since the earnings of men who go to college are greater than the earnings of men who stop their schooling with high school, the dollar increases associated with completing college are greater than those associated with finishing high school in all data sets.

Most of our data sets suggest that with background or ability controlled, the estimated effects of four years of high school fall more than the estimated effects of four years of college. Consequently, the PSID, Parnes, Veterans, and Kalamazoo data suggest that for men who are initially similar, four years of college raise earnings by a larger percentage than four years of high school.

These findings suggest that either (1) college completion is associated with larger unmeasured differences in initial ability or motivation than high school completion, or (2) college augments productivity more than high school does, or (3) employers irrationally "over-reward" college credentials. Because the coefficient for holding a BA is especially insensitive to controls for background and ability in the OCG and PSID data, I think it is improbable that measures of other kinds of characteristics would reduce the apparent effect of having completed college. If college augmented productivity more than high school does, I would expect the effect of an average year of higher education to be larger than the effect of an average year of secondary school. Since it is not, and since I cannot conceive of unique effects of the senior year that enhance individual productivity, I conclude that employers favor college graduates even when they are quite similar to nongraduates. This may not, of course, be irrational in all instances. On the average, college graduates may be sufficiently superior workers to economically warrant their favorable treatment.

Age differences in the Effects of Schooling on Ln Earnings

Our evidence on the effects of education for men of varying ages is difficult to interpret (See Table 6.8). This is because observed intercohort differences in the effects of schooling may arise because of age differences, cohort differences, differences associated with cohorts at particular ages, secular trends, and sampling error. Bartlett's analysis of Census data from 1939 to 1969 in Chapter 7 suggests that wage equations were quite stable from 1949 to 1969, implying few cohort differences. Nevertheless, for some levels of schooling and experience there appear to be cohort differences (or secular trends) in her data.[25]

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[25] For example, the income advantage of 45-54 year old college graduates over 45-54 year old high school graduates rose from 55 percent in 1949 to 69 percent in 1959 to 77 percent in 1969 (see Appendix J, Tables 2 and 4).

Table 6.8 Effects of Education on Natural Logarithm of Earnings on Income Stratified by Age

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1970 Census						
25-29 (N=3748)	1.	.0951 (.0109)	[-.0241] (.0191)	[.0308] (.0613)	.626	Experience Experience^2
30-34 (N=3375)	2.	.0841 (.0088)	[-.0146] (.0174)	[.0554] (.0615)	.575	Same as Equation 1
35-44 (N=6963)	3.	.0884 (.0044)	-.0229 (.0108)	.1907 (.0463)	.614	Same as Equation 1
45-54 (N=6834)	4.	.0893 (.0044)	-.0286 (.0122)	.2062 (.0538)	.658	Same as Equation 1
55-64 (N-4777)	5.	.0602 (.0053)	[.0123] (.0183)	.1112 (.0835)	.739	Same as Equation 1
OCG 25-34 (N=3166)	6.	.1004 (.0071)	-.1120 (.0179)	.4173 (.0727)	.649	Measured Background[a], Experience
35-44 (N=3443)	7.	.0862 (.0058)	-.0758 (.0177)	.3197 (.0770)	.642	Same as Equation 1
45-54 (N=2951)	8.	.0735 (.0074)	[-.0352] (.0247)	[.0783] (.1125)	.782	Same as Equation 1
55-64 (N=1944)	9.	.0951 (.0088)	[-.0481] (.0353)	[.1024] (.1590)	.800	Same as Equation 1

Table 6.8 Continued (2)

Sample		Years of Education	Years Higher Education	EA	Standard Deviation of Residuals	Other Variables Controlled
Michigan PSID 25-34	10.	.1223 (.0213)	[-.0393] (.0382)	.0695 (.1195)	.522	Measured Background,[b] Vocational Testing, Experience
35-44 (N=528)	11.	.0708 (.0177)	[.0574] (.0392)	[-.0337] (.1500)	.572	Same as Equation 5
45-54 (N=431)	12.	.0582 (.0157)	[-.0403] (.0402)	.4376 (.1585)	.527	Same as Equation 5
55-64 (N=270)	13.	[.0130] (.0324)	[.0045] (.0954)	[.4181] (.4014)	.962	Same as Equation 5
Productive Americans 25-34 (N-290)	14.	.0914 (.019)	[.0176] (.047)	[-.0936] (.161)	.532	Measured Background,[c] Vocational Training, Experience
35-44 (N=338)	15.	.0768 (.017)	[-.0004] (.046)	[.0811] (.163)	.605	Same as Equation 9
45-54 (N=331)	16.	.0498 (.021)	[.0107] (.067)	[.0788] (.260)	.775	Same as Equation 9
55-64 (N=229)	17.	.1079 (.028)	[.0414] (.101)	[-.1946] (.429)	.883	Same as Equation 9
Kalamazoo Brothers Under 45 (N=279)	18.	.0728 (.0346)	[-.0408] (.0441)	[-.0169] (.1348)	.438	Measured Background,[d] Test Score
45 and over (N=413)	19.	[.0448] (.0231)	[.0011] (.0325)	[.0783] (.1032)	.377	Same as Equation 13

Table 6 .8 Continued (3)

a. Father's Education, Father's Occupation, Father White Collar,
No Male Head, Nonfarm, NonSouth, Siblings, Race

b. Father's Education, Father's Occupation, Father White Collar,
No Male Head, Nonfarm, NonSouth, Siblings, Race

c. Father's Education, Nonfarm, NonSouth, Siblings, Race

d. Father's Education, Father's Occupation, No Male Head, Siblings

Since the effects of a high school education appear to be reduced more than the effects of a college education when test scores are controlled, and since ability differences seem to have larger effects among men over 30 than among younger men, I would prefer to rely on the PSID results for inter-cohort comparisons. But as Table 6.8 shows, the PSID results have large sampling errors. Moreover, for men under 35 and over 55 the relationships between our measures of education in Ln Earnings with no other variables controlled are quite different in the PSID from the relationships in the 1970 Census. These discrepancies preclude the use of the PSID to make general inferences about the effects of controlling ability or background on education coefficients for men of varying ages.

The OCG data are also discrepant from the Census data in that they suggest that the proportionate effects of a college education are lower for men over 45 than for men younger than 45. The PSID data agree with the Census data that the effects of a college education are smallest among men under 35.

Population coverage, question design, and coding procedures all differ from sample to sample. If one also allows for sampling error, I consider it fruitless to draw any firm conclusions from Table 6.8.

Ability Differences in the Effects of Education on Earnings

If more able men learn more and faster during a given educational experience than less able men, and if the economic benefits of educational attainment depend on learning, I would expect the measured effects of schooling to be greater for men with high test scores than for men with low scores. I would also expect more able men to compound their initial advantages as they continued in school. Ability differences would then have greater effects among better educated men than among less educated men. Our data do not support these expectations.[26]

Table 6.9 shows that their are few significant differences between schooling coefficients across ability groups in any of our samples. Moreover, the patterns of observed differences among ability groups are not consistent across samples.

Jencks also looked at ability effects within educational levels for the Veterans sample, and I did so for the Kalamazoo sample. We found

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[26] Nor do other data. The effects of measured ability show inconsistent and insignificant differences across schooling levels in the NBER-Thorndike, Rogers, Talent 5-Year Followup, and Husen samples analyzed by Hause (1972). Hause interpreted his findings as demonstrating an ability-schooling interaction, but I do not believe the data he reports sustain his conclusions.

Weisbrod (1972) called attention to the possible omission of measures correlated with both ability and schooling in Hause's analysis, e.g. motivation. This would not in itself bear on the question of an ability-education interaction. However, if an omitted variable bore a different relationship to ability across several levels of education, it could account for an apparent ability-education interaction. For example, if motivational differences between ability levels are greater among better educated men than among less educated men, and if, as Weisbrod suggests, motivation and ability are negatively correlated within educational levels, then the differences between the actual ability coefficients across educational levels would be larger

Table 6.9 Effects of Education on Natural Logarithm of Earnings Stratified by Test Score (Q)

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
PSID						
Q =1-9 (N=764)	1.	.0416 (.0107)			.735	Measured Background,^{a} Test Score, Experience
	2.	.0438 (.0132)	[-.0927] (.0507)	.5771 (.2294)	.732	Same as Equation 1
Q =10-11 (N=707)	3.	.0772 (.0093)			.606	Same as Equation 1
	4.	.0868 (.0156)	-.0689 (.0347)	.2759 (.1247)	.605	Same as Equation 1
Q =12-13 (N=303)	5.	.1020 (.0142)			.594	Same as Equation 1
	6.	.0966 (.0315)	[-.0094] (.0527)	[.0373] (.1667)	.596	Same as Equation 1
Kalamazoo Brothers						
Q Less than 90 (N=168)	7.	.0753 (.0178)			.370	Measured Background^b Test Score
	8.	.0881 (.0268)	[-.0655] (.0631)	[.2682] (.2590)	.371	SAme as Equation 7
Q =90-110 (N=349)	9.	.0356 (.0115)			.434	Same as Equation 7
	10.	[.0370] (.0334)	[.0273] (.0451)	[-.1701] (.1318)	.435	Same as Equation 7
Q Over 110 (N=175)	11.	.0483 (.0117)			.362	Same as Equation 7

Table 6.9 Continued (2)

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
Q Over 110 (N=175)	12.	[.0355] (.0870)	[-.0215] (.0921)	.2155 (.1097)	.360	Same as Equation 7
Talent 28 Year Olds						
Less than 90 (N=173)	13.	[.0151] (.0247)			.380	Measured Background[c] Test Score, Experience
90-110 (N=395)	14.	.0540 (.0109)			.362	Same as Equation 13
Over 110	15.	.0484 (.0173)			.405	Same as Equation 19
Veterans						
Q Less than 96 (N=236)	16.	.1015 (.0299)			.487	Measured Background,[d] AFQT, Experience
	17.	.1064 (.0313)	[-.0761] (.0662)	[.249] (.3335)	.487	Same as Equation 16
Q=96-103 (N=264)	18.	[.0124] (.0239)			.413	Same as Equation 17
	19.	[-.0016] (.0318)	[.0221] (.0431)	[-.0068] (.1670)	.414	Same as Equation 17
Q Over 103 (N=303)	20.	.0516 (.0219)			.426	Same as Equation 17
	21.	[.0497] (.0460)	[-.0071] (.0528)	.0534 (.1084)	.427	Same as Equation 17

Table 6.9 Continued (3)

a. Father's Education, Father's Occupation, Father White Collar, No Male Head, Nonfarm, NonSouth, Siblings, Race

b. Father's Education, Father's Occupation, No Male Head, Siblings

c. Father's Education, Father's Occupation, No Male Head, Siblings, Race

d. Father's Education, Father's Occupation, No Male Head, Nonfarm, NonSouth, Race

Note: for test score descriptions and distributions see Table 6.5.

no consistent and few significant differences in ability coefficients across educational levels.

Because high ability men earn more on the average, the absence of a negative ability-schooling interaction with respect to Ln Earnings does indicate that the dollar returns to increased schooling may be significantly higher among high scores than among low scores.

The absence of an ability-education interaction is theoretically puzzling to economists, since it raises the question of why able students invest more in education (i.e. stay in school longer) if the payoff is no higher than for mediocre students. From a psychological perspective, however, this poses no serious problem since the psychological costs of staying in school are presumably greater for mediocre students.

Differences by Father's Occupation in the Effects of Education on Earnings

Our evidence on the differential effects of schooling for men from varying social backgrounds is also in accord with previous work. It shows no consistent differences among men from white-collar, blue-collar, and farm backgrounds (See Table 6.10).[27]

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[27] Hauser (1972) divided OCG and Wisconsin High School Senior respondents by farm background, and father's Duncan score for nonfarm men. He found no consistent differences in the effects of schooling on Ln Income or Ln Earnings in either sample.

Table 6.10. Effects of Education on Natural Logarithm of Earnings or Income Stratified by Father's Occupational Group

Sample		Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
OCG						
White Collar (N=2631)	1.	.0502 (.0115)	[-.0230] (.0206)	.2512 (.0676)	.686	Measured Background,[a] Experience, Experience^2
Blue Collar (N=4915	2.	.0762 (.0046)	-.0750 (.0157)	.2600 (.0706)	.628	Same as Equation 1
Farm (N=3288)	3.	.0863 (.0062)	-.0825 (.0313)	.3821 (.1493)	.831	Same as Equation 1
PSID						
White Collar (N-329)	4.	.1377 (.0355)	[-.0578] (.0547)	[.0392] (.1486)	.575	Measured Background,[a] Test Score, Vocational Training, Experience, Experience^2
Blue Collar (N=862)	5.	.0320 (.0140)	[.0075] (.0364)	[.2555] (.1417)	.661	Same as Equation 4
Farm (N=583)	6.	.0595 (.0141)	[-.0137] (.0453)	[.3343] (.1867)	.645	Same as Equation 5
Talent 28 year olds						
White Collar (N=448)	7.	.038 (.011)			.355	Measured Background,[b] Test Score, Experience, Experience ^{2}
Blue Collar (N=315)	8.	.060 (.016)			.402	Same as Equation 7

Notes for Table 6.10

a. Father's Education, Father's Occupation, No Male Head, Nonfarm, NonSouth, Siblings, Race

b. Father's Education, Father's Occupation, No Male Head, Siblings

Caveat on Measurement Error

This chapter has emphasized omitted variables as a source of upward bias in the observed effects of schooling on occupational status and earnings. It has ignored a well-known source of downward bias, namely measurement error. If education is measured inaccurately, the effects of education will be underestimated. This may remain true when cognitive skills and family background are controlled. The extent of the bias depends on the relationships among errors in measurement, and among errors and the true values of variables, as well as on the effects of still omitted variables affecting both schooling and income.

I have ignored the effects of measurement error because I did not have the data I would need to correct for it. The accuracy of measurement varies from survey to survey, so reliabilities from one sample may not apply to others. Few of our data sets have multiple measures of variables which are essential to estimating reliabilities for correlations, and none of our data sets include information which allow us to confidently estimate the relationships between errors in measurement and true values, which are necessary for estimating true variances.

What evidence we do have, along with recent work elsewhere in assessing the consequences of measurement error, suggests that the effects of education are not seriously biased by ignoring measurement error. Bielby, Hauser, and Featherman (1976) indicate that errors in measuring parental socioeconomic status and education in the 1973 OCG impart a 10 percent downward bias to the schooling coefficient in their equation predicting occupational status. The difference between the corrected and uncorrected coefficients is only 4.91 - 4.39 = 0.52 points.[28]

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[28] Bielby, Hauser, Featherman (1976), Tables 7 and 8.

My corrections for measurement error in the Kalamazoo data suggest that the true standardized effect of education on dollar earnings, controlling sibling test score differences and family background common to brothers is 0.226. The effect without correcting for measurement error is 0.220.[29/]

Bishop (1976) has noted that the use of sibling data can exacerbate the problem of measurement error, and has argued that the within-pair unstandardized effect of schooling on earnings is at a maximum only 83 percent of the true effect. However, the accuracy of educational reports in the Kalamazoo data appears to be slightly higher than in the CPS data Bishop analyzed.[30/] My results indicate that if there were no other omitted variables, the observed within-pair coefficient of education for earnings could be 89 percent of the true coefficient.[31/]

29/ Olneck (1976), page 196.

30/ Bishop estimated the correlation between reported and true values as 0.90, assuming that errors in separate reports of education are correlated 0.40 (Bishop, 1976; p. 5). I estimated the correlation between true and reported values of education in the Kalamazoo data as 0.964 (Olneck, 1976; pp. 172-178).

31/ I calculated the error variance of schooling is $(2.73)^2 (1 - 0.964^2) = 0.527$. Bishop gives the ratio of the true to the observed coefficient

as $b_t / \beta = [1 - \frac{2V(ui)}{V(\ P)}]/\alpha$

β = true coefficient

b_t = observed coefficient

α = correction for floor and ceiling effects producing a correlation between the errors in measurement and true values.

$V(u_i)$ = error variance in education

However, the Kalamazoo sample also includes an ability measure. The bias in the within-pair education coefficient due to measurement error therefore depends on both the relative error proneness of the schooling and ability variables and on the sibling correlations of these variables. Since the ratio of error variance to the variance of sibling differences in education appears to be smaller than the analagous ratio for test scores, adding test score differences reduces the remaining downward bias in the within-pair education coefficient.[32] Therefore the observed coefficient of 0.0310 for Ln Earnings in the Kalamazoo data is quite likely close to 90 percent of the true coefficient. These calculations do not suggest

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31/ (continued)
$V(\Delta P)$ = variance of sibling differences in education. Adopting Bishop's values of = 0.95, I have $b_t/\beta = [1 - (2)(0.527)/6.720]/0.95 = .888$.

[32] Assuming random errors and a reliability of 0.929, the error variance in schooling is $(2.73)^2(1-0.929) = 0.5270$. The ratio of error variance to the variance of sibling differences is $0.0527/6.7288 = 0.0783$. If errors in test scores are random, assuming a reliability of 0.900 yields an error variance of $(15.32)^2(1-0.900) = 23.3292$. The ratio of error variance in test scores to the variance of sibling differences is $23.3292/249.5294 = 0.0935$ (See Bishop, 1976).

that my conclusions regarding the effects of education would be substantially altered by corrections for measurement error. Since such corrections are problematic and arbitrary, ignoring them seems reasonable.

Effects of College Quality

Individuals often try to go to a good school because they believe that going to a good school leads to higher economic benefits. But individuals who go to good schools are usually also the "right kind of material." Sorting out the effects of school resources, characteristics of classmates, and individual characteristics is difficult. Research on the effects of college quality is plagued by the confounding of these factors.

The Productive Americans survey rated the colleges respondents attended by a selectivity index that is divided into Unaccredited, Non-selective, Selective, Highly Selective, and Very Highly Selective categories.[33] The index is based on the ratio of acceptances to applicants, freshman test scores, freshman high school rankings, and similar data. It does not separate student characteristics from institutional resources.

For men with similar background in the PA, differences in college selectivity bear no significant relationship to occupational attainment.[34] Indeed, men from nonselective colleges have a slight occupational advantage over men from more selective colleges. College selectivity did, however, affect earnings. 14.3 percent of all PA respondents reported having graduated from college. SRC classified roughly half those men as having attended a "selective" college. These men earned an average of 28 percent more than men who had graduated from what SRC classified as an "unselective" college. This advantage persisted with both occupational status and weeks worked controlled. The earnings differences between men from colleges classified as "selective", "highly selective," and "very highly selective" were statistically insignificant.

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[33] See McClelland, Appendix C for a description of the index.

[34] Tables 14a and 16a, Appendix C.

I suspect that if we could control individual ability, our estimates of the earnings effects of college selectivity would fall substantially, and perhaps even be negative. In a subsample of 1957 Wisconsin high school seniors who attended college, only five percent of the variance in 1967 earnings lay between twelve categories of colleges type. Controlling socioeconomic background and 10th grade aptitude test score reduced the amount of between-college type earnings variance to 2-3 percent. Moreover, increased college prestige bore no consistently positive relationship to earnings at age 27.[35]

Effects of High School Curriculum

Taken alone, assignment to a college track is associated with large and significant advantages on both occupational status and earnings. However, once socioeconomic background and AFQT are controlled, the effects of track assignment on both outcomes are small and insignificant.[36]

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[35] See Alwin, Hauser, and Sewell in Sewell and Hauser (1975).

[36] Since most respondents took the AFQT after completing their schooling, a skeptic could argue that track assignment affects test scores, and that controlling AFQT is consequently illegitimate. However, analyses of Project Talent high school data suggest that changes in test scores from 9th to 12th grade which are related to track placement are quite small (Jencks and Brown 1975).

Conclusions

The actual effects of schooling depend on the level of schooling, the measure of economic success, and the population studied. The estimated effects of schooling also depend on the range of causally prior variables a researcher can control and on the amount of measurement error.

Completing high school rather than elementary school is associated with an occupational advantage of just under half a standard deviation in our national samples of 25-64 year olds. With background and test scores controlled the advantage appears to be more like a third of a standard deviation. This advantage does not appear to vary systematically by age, race, father's occupation, or test scores.

Completing college rather than high school is associated with an occupational advantage of more than one deviation in our national samples of 25-64 year olds. This advantage is not appreciably reduced when we control background and test scores. The advantage appears to be larger for nonwhites and for men whose fathers were farmers than for other groups. The advantage does not vary systematically with test scores. The advantage is larger among younger men, suggesting that it is increasing over time.

Completing college increases earnings by about the same percentage as completing high school. With experience controlled, an extra year of education is associated with 8 to 11 percent more earnings in our national samples of 25-64 year olds. With background controlled the advantage falls to between 6 and 9 percent. The effects of controlling test scores are more problematic. We do not have appropriate scores for national samples of 25-64 year old men. In the samples with appropriate scores, both the dispersion and the bivariate effect of schooling on earnings are smaller than in national samples of 25-64 year olds. Controlling both background and test scores reduces the apparent effect of education on earnings in these samples by 0.01 to 0.02 more than controlling background alone . The estimated true benefits of additional education in these samples are thus about half the observed association. If we take this to imply a similar absolute change in the coefficient of education for national samples of 25-64 year olds, the true benefit of an extra year of education could be anywhere from 4 to 8 percent. If we assume that controlling test scores would reduce the coefficient of education by the same proportion among 25-64 year olds as in our best restricted samples, returns would be between 4 and 6 percent.

With background and test scores controlled, returns to four years of college may be somewhat higher than returns to four years of secondary school. Certainly the absolute dollar value of college is greater. Returns do not seem to vary significantly by race, father's occupation, or adolescent test performance.

Chapter 7

CHANGES IN THE EFFECTS OF EDUCATION AND EXPERIENCE ON INCOME: 1939 - 1969

by Susan Bartlett

Most studies of the relationship between education and income rely on cross-sectional data. Many contemporary theories about the relationship are, however, best tested with historical data. Freeman (1975), for example, has used trend data for 1969-1973 to support a supply and demand model of wage determination. He found that the ratio of college to high school graduates' average incomes (for all workers 25 and over) dropped 6 percent between 1969 and 1973. He also found that the supply of college graduates exceeded the demand during this period. He inferred that this excess supply caused the relative wages of educated workers to fall. In this paper I will study changes in the returns to education between 1939 and 1969, and will try to determine whether the observed changes can be explained using a similar supply and demand model.

I am not the first to investigate this question. Miller (1966: 156-161) used Census and Current Population Survey data to argue that income differentials among men with varying amounts of education remained relatively constant between 1939 and 1959. But as I show below, his conclusion is probably wrong because his 1939 sample is significantly different from the samples he used in other years.

Carnoy and Marenbach (1975) also estimated changes in the rates of return (i.e. cost/benefit ratios) to education between 1939 and 1969 using Census data. Their estimates depend, however, on complex and problematic assumptions about the "social" and "private" costs of schooling. In addition, Carnoy and Marenbach recognized only some of the differences between the 1939 sample and the samples for other years.

In this paper I will focus only on the benefits of education (i.e. total annual income or annual wage and salary income). I will also make more systematic efforts to ensure that my data for different years are really comparable.

Methods

The Census first collected information on the educational attainment and income of individuals in 1940. It has published cross-tabulations of income by education and age for each decennial Census since then. The published tables for 1960 and 1950 are similar to one another, though not quite identical. The published tables for 1940 are drastically different from those for subsequent years. They cover only wage and salary income and only men who received less than $50 from sources other than wages and salaries. The tables for different years also cover different geographic and ethnic groups, treat non-respondents differently, and group respondents differently.

Ideally, one would like to construct comparable samples for each Census using the original data. Unfortunately, these data are not available for 1940 and 1950. To achieve comparability, I replicated the variable definitions, the treatment of missing data, and the sample coverage of earlier Censuses using the 1970 Census 1/1000 Public Use Sample.1/ This gave me three different 1970 samples, each comparable to one earlier Census but not to the other two. I then used the 1970 data to create three tables

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1/ See U.S. Bureau of the Census, *Public Use Samples of Basic Records from the 1970 Census: Description and Technical Documentation*. U.S. Government Printing Office, Washington, D.C., 1972. I used the 1/1000 county groups sample which is actually a 1/50 sample of the Census Bureau's "5 percent sample."

that replicated the published tables from the 1940, 1950 and 1960 Censuses as closely as possible. To create 1970 tables with income categories as comparable as possible to those of other years, I divided all 1969 incomes by the ratio of mean 1969 income to mean income in the earlier Census. I then grouped the 1969 data into categories identical to those used in the earlier Census. All comparisons excluded respondents with values of zero on the income measure. The 1950 comparison also excluded respondents with negative income. The 1960 tables grouped men who received negative income with those who received $1-$499 so I could not eliminate them. The 1940 tables do not have any negative values, since they cover only wage and salary income. Appendix J describes the variables, the populations covered, the treatment of non-respondents, and the categorization of variables in more detail. It also shows the frequency distribution of age, education, work experience and income for each year and compares them to a similarly defined 1970 sample. Finally, it presents breakdowns of income by years of education and age for each of the comparisons. The breakdowns provide the basic data for subsequent analyses.

I estimated work experience from age and education. I then used educational attainment and years of work experience to predict the natural log of income (Ln Income) for males aged 25 to 64. The coefficient of education in this equation obviously overestimates the true effect of schooling on income. Chapter 6 shows, for example, that controlling family background and adolescent test score reduces the coefficient of education by 35 to 70 percent. But Hauser and Featherman (1976) show that the correlation between education and family background was quite stable for men born between 1897 and 1947. And Crouse (Chapter 3) concludes that the correlation between education and test scores has also been quite stable. The percentage bias due to not controlling background and test scores should therefore be fairly stable from 1939 to 1969. Observed changes in the coefficient of education with experience controlled thus imply proportional changes in the economic benefits of education.

The coefficients of education and experience in my equations measure the percentage increase in income associated with an extra year of schooling or experience. Mincer (1974) has argued that the costs of education are roughly offset by an individual's earnings while in school, that earnings profiles for men with different amounts of schooling are roughly parallel once they enter the labor force, and that an extra year of education increases mean retirement age by roughly one year. He shows that if these assumptions are correct, the coefficient of education (with experience controlled) estimates the average rate of return to an additional year of schooling. Mincer also assumes that all age-related changes in earnings are due to on-the-job training, not mere aging. These assumptions may or may not be accurate. Nonetheless, I will describe my results as measuring "returns" to schooling and experience, simply because this is the way many people think about the problem, and it provides a convenient way of describing the results.

Findings

Overall Inequality: Table 7.1 shows that the mean educational attainment and mean wages of men aged 25-64 increased between 1939 and 1969. The standard deviation of education fell. The standard deviation of wages rose, but not as much as the mean. The coefficient of variation of Wages therefore fell from 0.846 to 0.620. The standard deviation of Ln Wages fell from 0.822 to 0.622. The

Table 7.1

Means and Standard Deviations of Variables for Males Aged 25-64 in Six Census Samples.

		1940 Census Definitions		1950 Census Definitions		1960 Census Definitions	
		1939	1969	1949	1969	1959	1969
Education	Mean (S.D.)	8.860 (3.535)	11.305 (3.410)	9.433 (3.784)	11.323 (3.607)	10.253 (3.752)	11.279 (3.601)
Work Experience	Mean (S.D.)	22.963 (11.215)	23.010 (12.066)	25.157 (12.031)	24.822 (12.334)	25.381 (11.980)	24.868 (12.336)
Annual Income*	Mean (S.D.)	1324 (1119)	8739 (5417)	3256 (2539)	9574 (7223)	5716 (4352)	9494 (7172)
Ln Income*	Mean (S.D.)	6.880 (.822	8.904 (.622)	7.814 (.811)	8.911 (.782)	8.378 (.813)	8.896 (.789)
N		790,509	26,291	1,140,665	40,248	1,966,052	42,192

* Mean 1969 Annual Income is slightly different from the mean I used to calculate the deflation factor. This is true because: 1) the mean I used to calculate the deflation factor was from a 1/10 subsample of the 1970 1/1000 Public Use Sample and 2) the mean and standard deviation in this table reflect the effects of using grouped income data as opposed to ungrouped data.

The 1940 Definition is Annual Wage and Salary Income, for men with positive and primarily wage and salary income. The 1950 definition included men with positive income. The 1960 definitions included men with non-zero income.

fact that both the coefficient of variation and the standard deviation of Ln Wages declined substantially indicates that the distribution of wage and salary earnings for men with no other major source of income was significantly more equal in 1969 than in 1939.

Table 7.1 also suggests that this decline took place largely between 1939 and 1949. The reduction in inequality between 1949 and 1969 is relatively minor, and occurred after 1957.[2/]

The reduction in inequality between 1939 and 1949 would not have been apparent if we had looked only at published Census tables. The standard deviation of Ln Income in the published Tables is 0.822 for 1939, 0.811 for 1949, and 0.813 for 1959. The reduction in inequality becomes apparent only when we take account of the fact that the published tables for 1939 cover a restricted sample, whose incomes were appreciably more equal than those of the population as a whole.[3/]

[2/] Miller (1966: 15-26 and 75-79) also reports that inequality of both wages and total family income decreased during the 1940's and remained essentially constant during the 1950's. CPS data suggest that inequality in the distribution of earned income remained almost constant between 1949 and 1969 (Henle, 1972).

[3/] In 1969 the standard deviation of Ln Income was 0.789 for all men with non-zero income compared to 0.622 for men with non-zero wages and negligible income from other sources. We do not, of course, know the standard deviation of Ln Income for all men in 1939. My inference that inequality fell could be wrong if the elimination of men with appreciable non-wage incomes reduced the standard deviation more in 1969 than in 1939. In 1969 the restriction eliminated both high income men with substantial assets and low income men with substantial transfers. It left the mean of Ln Earnings almost unchanged but appreciably reduced the mean of dollar Earnings. The restriction would probably not have eliminated as many men with transfers in 1939, but it would have eliminated more low income farmers.

Effects of Education and Work Experience. Equation 1 in Table 2 shows that each additional year of education increased Wages by 9.4 percent in 1939 compared to only 6.5 percent in 1969.[4/] Equation 2 shows the effect of adding Experience and Experience^2 to the regression. The implied returns to schooling for wage workers now declines from 11.9 percent in 1939 to 7.7 percent in 1969. The coefficient of Experience declined from 0.057 in 1939 to 0.036 in 1969. The coefficient of Experience^2 increased from -0.00087 in 1939 to -0.00061 in 1969. This indicates that the experience profiles were flatter in 1969 than in 1939. The implied benefits of the early years of experience thus declined between 1939 and 1969.

Equation 3 drops the assumption that returns to Experience are the same at every level of education and adds two multiplicative interaction terms, Education x Experience and Education x Experience^2. The positive coefficient of/Education x Experience and the negative coefficient of/Education x Experience^2 indicate that years of work experience increased Ln Wages more for the highly educated than for the less educated. The observed changes in the coefficients of these interactions between 1939 and 1969, though statistically significant, are not large enough to be very interesting.

The data show that the wage differentials among men with varying amounts of schooling and work experience were substantially less in 1969 than in 1939. When did these changes occur?

Equation 2 in tables 3 and 4 shows that returns to schooling increased from 8.9 percent in 1949 to 9.6 percent in 1959 and then fell to 9.4 percent in 1969.[5/] The changes in the coefficients of Experience,

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[4/] I also ran a regression that allowed each level of schooling to have a different impact on Wages. This raised R^2 by only 0.004 in 1939 and 0.003 in 1969, and provided no new substantive insights. I will therefore discuss only the linear equations.

[5/] The regressions that allowed each level of schooling to have a different

Table 7.2

Regressions of Ln Wages on Education and Experience for Males Aged 25-64 With Primarily Wage and Salary Income: Census Surveys, 1939 (N=790,509) and 1969 (N=26,291)

	Year		Years of Education	Experience	Experience^2	Education x Experience	Education x Experience^2	Constant	R^2	S. D of Residual
(1)	1939	B	.09400					6.04748	.1633	.75228
		(S.E.)	(.00024)							
	1969	B	.06462					6.33183	.1253	.58211
		(S.E.)	(.00105)							
(2)	1939	B	.11850	-.05724	-.00087			5.08246	.2044	.73357
		(S.E.)	(.00076)	(.00034)	(.00001)					
	1969	B	.07667	.03637	-.00061			5.76865	.1568	.57156
		(S.E.)	(.00119)	(.00126)	(.00002)					
(3)	1939	B	.12860	.05353	-.00084	[.00009]	-.00002	4.91491	.2056	.73302
		(S.E.)	(.00097)	(.00095)	(.00002)	(.00009)	(.00000)			
	1969	B	.04967	[-.00031]	.00018	.00340	-.00008	6.08205	.1610	.57017
		(S.E.)	(.00440)	(.00478)	(.00008)	(.00036)	(.00001)			

Table 7.3

Regressions of Ln Income on Education and Experience for Males Aged 25-64 With Positive Income: Census Surveys, 1949 (N=1,140,665) and 1969 (N= 40,248)

ear			Years of Education	Experience	Experience^2	Education x Experience	$\text{Education x Experience}^2$	Constant	R^2	S.D. of Residual
1)	1949	B	.07709					7.08675	.1294	.75661
		(S.E.)	(.00019)							
	1969	B	.08591					6.87132	.1572	.71756
		(S.E.)	(.00099)							
2)	1949	B	.08928	,0[illegible]316	-,00070			6.42776	.1503	.74749
		(S.E.)	(.00021)	(.00027)	(.00001)					
	1969	B	.09380	.04758	-.00083			6.23990	.1873	.70464
		(S.E.)	(.00111)	(.00126)	(.00002)					
3)	1949	B	.06097	.01196	-.00003	.00353	-.00008	6.69719	.1535	.74609
		(S.E.)	(.00087)	(.00083)	(.00001)	(.00007)	(.00000)			
	1969	B	.05534	[.00206]	[.00002]	.00383	-.00008	6.72912	.1898	.70355
		(S.E.)	(.00460)	(.00487)	(.00008)	(.00036)	(.00001)			

Table 7.4

Regression of Ln Income on Education and Experience for Males Aged 25-64 with Non-Zero Income: Census Surveys, 1959 (N=1,966,052) and 1969 (N=42,192)

Year		Years of Education	Experience	Experience^2	Education x Experience	Education x Experience^2	Constant	R^2	S.D. of Residual
(1) 1959	B	.08777					7.47814	.1642	.74310
	(S.E.)	(.00014)							
1969	B	.08518					7.44257	.1511	.72706
	(S.E.)	(.00098)							
(2) 1959	B	.09648	.04152	-.00070			6.88385	.1827	.73479
	(S.E.)	(.00016)	(.00020)	(.00000)					
1969	B	.09304	.04697	-.00082			6.81761	.1798	.71468
	(S.E.)	(.00110)	(.00125)	(.00002)					
(3) 1959	B	.07099	.01205	-.00009	.00297	-.00006	7.15798	.18468	.73392
	(S.E.)	(.00068)	(.00068)	(.00001)	(.00005)	(.00000)			
1969	B	.05932	[.00602]	[-.00003]	.00355	-.00007	7.23530	.1821	.71367
	(S.E.)	(.00454)	(.00480)	(.00008)	(.00036)	(.00001)			

Experience^2, and the interactions do not seem large enough to evoke much interest. Taken together, these results suggest that there was a dramatic decline in the effect of both education and experience between 1939 and 1949, and that there was no clear trend from 1949 to 1969.

Decomposition of the Variance of Ln Income. The changes in the returns to education and experience can be summarized by decomposing the variance of income into components explained by education, by experience, by the covariance of education and experience, and by other unmeasured influences. If we denote Ln Income as Y, Years of Education as S, Years of Experience as X, Experience^2 as X^2, and the deviation of an individual's income from his predicted income as e, the second equation in Tables 7.2 to 7.4 is:

(1) $Y = B_o + B_1S + B_2X + B_3X^2 + e$

where B_1 and B_2 and B_3 are the coefficients of their respective variables and B_o is the constant. Taking variances and simplifying, the equation becomes:

$$(2) \quad \text{Var } Y = B_1^{\;2} \text{ Var }(S) + B_2^{\;2} \text{ Var }(X) + B_3^{\;2} \text{ Var }(X^2) + 2B_2B_3 \text{ Cov}(X,X^2) + 2B_1B_2 \text{ Cov}(S,X) + 2B_1B_3 \text{ Cov}(S,X^2) + \text{Var } e$$

Table 7.5 summarizes changes in the relationship between education, experience and income using equation 2.

1939 to 1969. The variance of both Education and Experience decreased slightly among wage earners between 1939 and 1969. The absolute effect of both variables on wages also fell substantially. The variance in Ln Wages explained by Education therefore fell by 61 percent. The variance explained by Experience also fell by 61 percent. The covariance between Education and Experience declined by 71 percent, and the variance due to other factors fell by 39 percent. The total variance of Ln Wages declined by 43 percent.

The variance of Ln Wages explained by Education is a function of the variance of Education and the regression coefficient of Education. The variance explained by Education declined from 0.1755 to 0.0683 between 1939 and 1969. Had the coefficient of Education remained constant, and only the variance of Education declined, the explained variance in Ln Wages would have fallen by $(3.535^2 - 3.410^2)/3.535^2 = 6.9$ percent. Had the distribution of Education remained constant, while the coefficient fell, the explained variance would have decreased by $(0.11850^2 - 0.07667^2)/0.11850 = 58.1$ percent. Clearly the change in the coefficient of Education contributed more to the decline in the explained variance than did the change in the variance of Education.

The dispersion of Experience increased slightly between 1939 and

Table 7.5

Decomposition of the Variance of Ln Income for Males Aged 25-64 from the 1939, 1949 and 1959 Comparisons

		(1) Variance Explained by Education	(2) Variance Explained by Experience	(3) 2* Co-variance	(4) Unexplained Variance	(5) Total Variance	(6) R^2
Samples Using 1939 definitions	1939	.1755	.0350	-.0721	.5381	.6763	.2044
	1969	.0683	.0137	-.0211	.3267	.3874	.1568
Samples Using 1949 definitions	1949	.1141	.0156	-.0305	.5587	.6576	.1503
	1969	.1145	.0193	-.0196	.4965	.6109	.1873
Samples Using 1959 definitions	1959	.1310	.0132	-.0229	.5399	.6606	.1827
	1969	.1122	.0187	-.0190	.5108	.6227	.1798

Note: From equation 2:

Column (1) = B_1^2 Var S

Column (2) = B_2^2 Var X + B_3^2 Var X^2 + $2B_2B_3$ Cov (X,X^2)

Column (3) = $2B_1B_2$ Cov (S,X) + $2B_1B_3$ Cov (S,X^2)

Column (4) = Var e

Column (5) = Column (1) + Column (2) + Column (3) + Column (4) except for small discrepancies due to rounding error in the SPSS computing routine.

1969, so the decline in the variance explained by Experience is entirely due to the decline in the coefficient of Experience.

1949 to 1969 The dispersion in educational attainment decreased slightly between 1949 and 1969, but the coefficient of Education rose. As a result, the variance explained by Education increased. The same is true of the variance explained by Experience. The residual variance fell by 11.1 percent during this period. The total variance declined by 7.1 percent.

1959 to 1969 The variance of Education decreased slightly during the 1960's. There was very little change in the effect of additional years of Education on Ln Income so the variance explained by Education decreased slightly. The total variance of Ln Income also declined slightly.

Explaining the Results

How can we explain the equalization of wages and the decline in the impact of education on wages between 1939 and 1949? According to traditional economic theory, individual earnings are determined by the relationship of supply to demand for workers with various skills. The stability of relative wages between 1949 and 1969 implies that the supply of educated workers rose at the same rate as the demand during this period. The fact that the wages of the highly educated fell relative to the wages of the poorly educated between 1939 and 1949 implies that the supply of educated workers rose more rapidly than the demand during the 1940's. Is this plausible?

Column 1 of Table 7.6 shows the estimated increase during each decade in the mean educational attainment of 25-64 year old men with income. Columns 2 and 3 show increase in the proportion of such men with 12 and 16 years of schooling respectively. The figures suggest larger increases in the supply

Table 7.6

Increases in Supply and Demand for Educated Workers: 1940 - 1970

	Supply Shift			Demand Shift Due to Industrial Mix		Supply Shift Less Demand Shift Due to Industrial Mix	
	(1) Mean Years of Schooling	(2) Proportion With 12 or More Years of Education	(3) Proportion With 16 or More Years of Education	(4) 12 or More Years of Education	(5) 16 or More Years of Education	(6) 12 or More Years of Education (Col 6 - Col 2)	(7) 16 or More Years of Education (Col 7 - Col 3)
1940's	.555	.0853	.0163	.0190	.0062	.0663	.0101
1950's	.864	.0876	.0302	.0306	.0192	.0570	.0110
1960's	1.026	.1344	.0447	.0267	.0225	.1077	.0222

Column 1 computed from Table 7.1. Estimate for 1960's = Col 6 - Col 5 from Table 7.1 Estimate for 1950's = (Col 4 - Col 3) - (Col 6 - Col 5). Estimate for 1940's = (Col 2 - Col 1) - (Col 4 - Col 3). Columns 2 and 3 are computed for same samples as Tables 7.1 - 7.5. 5.48 percent of wage earners aged 25-64 were college graduates in 1939 compared to 13.62 percent in 1969. 7.77 percent of men with non-zero income aged 25-64 had college degrees in 1949 compared to 15.56 percent in 1969. 10.79 percent of men aged 25-64 with non-zero income were college graduates in 1959 vs. 15.26 percent in 1969. (See appendix for these percentages.) This suggests that restricting the population to wage earners lowers the proportion of college graduates. If the ratio of wage earners with college degrees to men with non-zero income with college degrees was the same in 1939 as in 1969, (15.26) (5.48)/13.62 = 6.13 percent of all men with non-zero income in 1939 were college graduates. Thus, the "true" increase in the proportion of college graduates during the 1940's was 1.63 percent.

As a check on these estimates I also calculated the shifts for all 25-64 year old men in the labor force in 1940, 1950, 1960, and 1970, using the following sources:

(1) U.S. Bureau of the Census, U.S. Census of Population: 1940, Educational Attainment by Economic Characteristics and Marital Status. U.S. Government Printing Office, Washington, D. C., 1947 (Tables 17 and 18. I had to estimate the number of native white and Negro males aged 14-17 and 65 and over from other Census employment data.)

(2) U.S. Bureau of the Census, U.S. Census of Population: 1950. Vol. IV, Special Reports, Part 5, Chapter B, Education. U. S. Government Printing Office, Washington, D.C., 1953. (Table 9)

(3) U.S. Bureau of the Census, U.S. Census of Population: 1960. Subject Reports, Industrial Characteristics, Final Report PC (2) - 7f. U.S. Government Printing Office, Washington, D.C., 1967. (Table 21)

(4) U.S. Bureau of the Census, U.S. Census of Population: 1970. Subject Reports, Industrial Characteristics, Final Report PC (2) -7B. U.S. Government Printing Office, Washington, D.C., 1973. (Table 3) These data are for men 16 years and older.

5.06 percent of all men in the labor force had college degrees in 1939 compared to 6.92 percent in 1949, 9.72 percent in 1959, and 13.81 percent in 1969. Therefore, the proportion of male college graduates in the entire labor force increased by 1.86 percent during the 1940's, 2.80 percent during the 1950's and 4.09 percent during the 1960's. These figures do not, then, alter the picture given in the table.

Columns 4 and 5 cover all males in the civilian labor force aged 14 and over in 1940, 1950 and 1960 and males aged 16 and over in 1970. Thus, the figures in columns 6 and 7 are approximations.

of educated labor during the 1960's than during the 1950's, and larger increases during the 1950's than during the 1940's. Thus if supply/demand ratios are to explain why returns to schooling declined during the 1940's but not during the 1950's and 1960's, we must hypothesize a much more rapid rise in demand for educated labor during the 1950's and 1960's than during the 1940's.

The demand for educated workers can change either because of changes in the industrial mix or because of changes in demand within industries. Changes in the industrial mix are presumably exogenous, in the sense that they do not depend to any great extent on the supply of educated labor. An increase in the supply of educated workers could, of course, lower the average wage rate for industries which require educated workers and hence lower the cost of their products. This in turn could increase the relative demand for the products of those industries. I doubt, however, that changes in the supply of educated workers affect the industrial mix significantly. Changes within industries may be either endogenous or exogenous. They are exogenous to the extent that technological changes alter the relative efficiency of different mixes of educated and uneducated workers within industries. They are endogenous to the extent that they merely reflect changes in the amount of education a firm can demand for a job with a fixed wage relative to the mean.

First, I will show how changes in the industrial mix between 1939 and 1969 affected the demand for educated workers. Then, I will compare these changes in demand to the observed increases in the supply of educated workers. Using this information, I can make inferences about changes in the demand for educated workers within industries.

In order to determine the effect of changes in the industrial mix on the demand for educated workers I calculated two indices for each of the four Censuses. One index estimated the demand for college graduates. The other estimated the demand for men with 12 or more years of schooling. I first calculated the proportion of men in each industry with college degrees and the proportion with 12 or more years of schooling in 1969. I treated these proportions as indices of industries' relative demand for educated labor and assumed that the indices were stable over time. I then calculated the average value of both indices across all industries for 1939, 1949, 1959, and 1969.[6/] This average changes only insofar as the relative size of different industries changes. Columns 4 and 5 in table 7.6 show the effects of shifts in the industrial mix on the demand for educated workers, assuming no change in demand within industries. The rate of increase in the demand for college graduates rose consistently during the decades between 1939 and 1969. The rate of increase in demand for high school graduates was greatest during the 1950's.

Columns 6 and 7 show the difference between the change in the supply and the change in the between-industry demand for college and high school graduates generated by changes in industrial mix. Part of this residual undoubtedly reflects changes in the demand for educated workers within industries. It is impossible to accurately measure within-industry shifts in demand, but we can ask what shifts would be necessary for a supply and demand model of wage determination to explain the observed changes in the returns to education.[7/]

[6/] Freeman (1974: 302-303) used this same method to calculate the demand for college graduates between 1951 and 1973.

[7/] One way to attempt to measure within-industry changes in demand would be to look at changes in the occupational mix within a given industry. Some of these shifts could be caused by exogenous changes in demand, but some would merely reflect changes in the supply.

The returns to education were virtually stable during the 1960's (see table 7.5). Therefore, increases in the demand for educated workers must have equalled increases in supply. This implies that the within-industry demand for college graduates must have increased by about 2.22 percentage points and the demand for high school graduates by 10.77 percentage points between 1959 and 1969.

During the 1950's, the returns to education increased slightly. Thus, the increase in the demand for college graduates within industries must have been somewhat greater than the 1.1 percentage points required to keep demand in line with supply. The increase must have exceeded 6 percentage points for high school graduates.

The returns to education fell by approximately a third between 1939 and 1949. To explain this in terms of supply and demand, the increase in the supply of educated workers would have had to be significantly larger than the increase in demand. But even if we assume that there was no increase in the within-industry demand for educated workers during the 1940's, the supply increase only exceeded the demand increase by 1.0 **percentage points for college graduates and 6.6 percentage points** for high school graduates. Any increase in the within-industry demand for educated workers would imply an even smaller excess of educated workers. It is difficult to believe that such small supply surpluses could have caused a 35 percent decline in the returns to education during the 1940's.

There is another problem with the supply-demand model. The 1940's were the only period in which the variance of income changed significantly. The variance explained by education and the variance explained by experience both declined by 61 percent. This was almost entirely due to a decline in the coefficients of both variables. There is no obvious reason why the demand for both highly educated and highly experienced workers should have fallen by the same proportion between 1939 and 1949.

In addition, the residual variance fell by 39 percent between 1939 and 1949. The residual variance could decline for either of two reasons.

(1) The variances and covariances of the unmeasured characteristics that affect income, like test scores, personality traits and technical skills, could have fallen between 1939 and 1949. Since three-quarters of the men aged 25 to 64 in 1939 were still employed in 1949, it is hard to believe that the variance of these personal characteristics could have changed very much in the interval.

(2) The variances and covariances of these unmeasured characteristics could have remained constant but their coefficients could have fallen. This could have happened if the demand for these unmeasured characteristics fell between 1939 and 1949. But I can see no obvious reason to believe that the demand for all these unmeasured worker characteristics fell so dramatically during this period, and then remained virtually stable from 1949 to 1969.

Supply and demand do not, then, provide a very satisfying explanation for observed changes in the returns to education and experience between 1939 and 1969. Fortunately, there may be a more parsimonious explanation. Instead of assuming that earnings inequality depends on the degree of inequality in education, experience, and other personal characteristics, combined with the rates of return to these characteristics, one can assume that the degree of inequality in earnings depends on exogenous factors. One can then assume that returns to education, experience, and other personal characteristics depend on the overall degree of inequality rather than the other way round.

One exogenous factor that affected the distribution of wages (and income) is the unemployment rate, which was 17.2 percent in 1939, 5.5 percent in 1949, 5.5 percent in 1959, and 3.5 percent in 1969.[8/] Metcalf (1969) found that high unemployment was associated with high variance in earnings. This is partly because many people have no earnings for at least part of the year and partly because the lowest paid workers are more likely to be laid off. The wages of white collar workers seem to be less affected. Schultz (1972) provides further evidence for this explanation. He found that the variance of log earnings declined far less between 1939 and 1949 for men employed throughout the year than for all men. This suggests that part of the decline in the variance of income between 1939 and 1949(and between 1959 and 1969) can be attributed to the declines in unemployment.

Thurow (1972) also argues that the government consciously attempted to establish a more equal set of relative wages through the use of wage and price controls during World War II. According to Thurow, the definitions of what constituted "fair" wage differentials changed during this period, and the new standards became a permanent part of the system. He argues that the distribution of income has remained relatively constant since 1949 because the occupational hierarchy has remained stable and the standard of "fair" wage differentials within that hierarchy has not changed.

8/ U.S. Bureau of the Census, Historical Statistics of the U.S., Colonial Times to 1957. Washington, D.C. 1960 (p.73, Table D46-56), and Economic Report of the President, 1974. Washington: 1974 (p.279).

If one is willing to assume exogenous changes in the variance of earnings, it is relatively easy to see how such changes could alter returns to education and experience. Thurow argues, for example, that the best paid jobs require more skills and that these skills are mostly learned on the job. Employers rank applicants according to how quickly they think the applicants will learn to do a job. The best paid jobs go to those with the lowest estimated training costs. Employers use education to estimate these costs. They also use other factors.

If this model were correct, and if employers' judgments about the importance of education relative to other worker characteristics remained constant, the correlation between education and wages would remain essentially stable/ regardless of how the variance in wages changed. If the variance of wages changed due to exogenous factors such as the unemployment rate or changes in socially acceptable wage norms, the unstandardized coefficients of all worker characteristics would change by approximately the same proportion. This is essentially what happened between 1939 and 1969. The regression coefficients of both education and experience fell by about a third. The correlation of education with earnings fell only an eighth, from 0.404 to 0.354. This theory also explains why the coefficients of education and experience have remained essentially constant since 1949. So long as the variance of wages is constant and employers' judgments about the relative importance of various worker characteristics are stable, the coefficients of these characteristics will not change.

The data do not prove that returns to schooling are immune to supply and demand. Nor do they prove that the dispersion of wages is unrelated to inequality in the distribution of human capital. The data do, however, raise serious doubts about the general applicability of the supply-demand model that Freeman invokes to explain declines in returns to higher education after 1969.

Chapter 8

WHITE/NON-WHITE DIFFERENCES IN EDUCATION AND INCOME: 1949-1973

Joseph Schwartz and Jill Williams

Introduction

This chapter will examine white/non-white differences in annual earnings and years of education. We will analyze annual data from the Current Population Survey (CPS) plus the five national samples of mature men, i.e. OCG, PA, NLS, Census, and PSID. The present chapter is divided into four sections

Section 1 examines time trends in the distribution of income, using CPS data. It looks both at inequality between races and inequality within races, reexamining the traditional assumption that the distribution of income among whites is more equal than that among non-whites.

Section 2 compares our five national samples of mature men to CPS samples for the same years. Of course, when our samples differ from CPS, we have little basis for determining **which** better represents the target population. **Nonetheless, comparing our five** samples to a uniform criterion helps determine the degree of comparability among the five samples, which cover slightly different target populations and use slightly different measures of race, education, and income/earnings.

Section 3 compares the determinants of education for whites and non-whites in the Census, OCG, and PSID. This allows us to estimate the reasons for convergence or divergence between whites and non-whites from 1961 to **1971.**

Section 4 compares the determinants of earnings for whites and non-whites in the Census, OCG, and PSID.

1. Changes in the Effects of Race on Income from 1949 to 1973

Table 8.1 contains information on Income, Income$^{1/3}$, and Ln Income (in 1967 dollars) for white and non-white males aged 14 and over from the CPS surveys of 1949 through 1973.[1] The first page gives the usual summary statistics for Income and Income$^{1/3}$, along with the difference between the white and non-white means. The second page gives these statistics for Ln Income, plus two additional measures of within-race inequality; the coefficient of variation (V) and Atkinson's measure of **inequality (A) (see Atkinson, 1972). In using Atkinson's measure we assume that "utility" is a linear function of** Income$^{1/3}$. Because the income distribution is skewed to the right, changes in the mean and standard deviation of income are more sensitive to changes at the top of the distribution than to changes at the bottom. Because the logs of income are skewed to the left, they are more sensitive to changes at the bottom of the distribution than at the top.

Table 8.2 presents regressions of the data in Table 8.1 on the variables Race, Year, Year^2, and the interactions Race x Year and Race x Year^2. These regressions were weighted to **(0.2 for non-white and 1.8 for white distributions)** approximate the true proportions of the two races in the population. The regression coefficients in Table 8.2 can be interpreted as follows. The Race coefficient measures the average difference between whites and non-whites over the 25-year period. The Year coefficient measures the average annual change in the dependent variable. The Year^2 coefficient measures the average

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[1] Starting in 1967 the CPS changed the reported category from 'non-white' to 'black', but we have ignored the distinction. Blacks constitute about 90 percent of the non-white subsample. (See Appendix A for a comparison of blacks and other non-whites in the Census sample). It was necessary to use data for all men 14 and over (14+) rather than men 25-64 because that is all that is available in published CPS tabulations prior to 1967.

Table 8.1

PERSONAL INCOME (1967 DOLLARS) OF MALES WITH INCOME AGED 14 AND OVER, BY RACE, 1949-1973.

Year	INCOME (1967 dollars)					INCOME$^{1/3}$				
	White		Non-White*		Diff.	White		Non-white*		Diff.
	mean	s.d.	mean	s.d.		mean	s.d.	mean	s.d.	
1949	3726	3174	1915	1769	1811	14.397	4.427	11.570	3.579	2.827
1950	4072	3430	2158	1848	1914	14.836	4.545	11.998	3.910	2.838
1951	4179	3166	2253	1680	1926	15.070	4.259	12.322	3.471	2.748
1952	4433	4088	2403	2397	2030	15.319	4.378	12.518	3.649	2.801
1953	4541	3974	2407	1842	2134	15.338	4.685	12.489	3.769	2.849
1954	4548	4012	2349	2344	2199	15.339	4.636	12.205	4.159	3.134
1955	4772	4341	2525	2000	2247	15.564	4.762	12.585	4.046	2.979
1956	5083	4576	2627	2161	2456	15.872	4.903	12.724	4.141	3.148
1957	4937	4261	2633	2161	2304	15.721	4.867	12.692	4.225	3.029
1958	4953	4280	2640	2695	2313	15.681	5.003	12.595	4.386	3.086
1959	5297	4659	2747	2764	2550	16.052	5.056	12.745	4.391	3.307
1960	5370	4629	2958	2586	2412	16.051	5.222	13.073	4.499	2.978
1961	5569	5043	3014	2804	2555	16.181	5.408	13.169	4.465	3.012
1962	5654	4880	2987	2446	2667	16.338	5.303	13.229	4.318	3.109
1963	5735	4887	3222	2730	2513	16.411	5.331	13.490	4.595	2.921
1964	5908	5068	3525	3381	2383	16.530	5.459	13.890	4.637	2.640
1965	6055	5223	3375	2782	2680	16.671	5.500	13.742	4.506	2.929
1966	6366	5355	3650	2929	2716	16.996	5.476	14.134	4.562	2.862
1967	6523	5446	3775	3201	2748	17.130	5.528	14.223	4.755	2.907
1968	6676	5444	3949	3188	2729	17.278	5.564	14.467	4.792	2.811
1969	6854	5533	4029	3188	2825	17.409	5.650	14.604	4.698	2.805
1970	6765	5516	4101	3333	2664	17.302	5.705	14.648	4.793	2.654
1971	6771	5462	4090	3397	2681	17.314	5.699	14.587	4.913	2.727
1972	7128	5658	4411	3649	2717	17.647	5.740	14.984	4.926	2.663
1973	7201	5575	4494	3651	2707	17.716	5.763	15.085	4.939	2.631

* After 1967, this column is based on blacks only. Regressions (not presented here) show that the differences between non-white means prior to 1967 and black means for 1967-1973 are entirely attributable to overall time trends for each of the five variables.

Table 8.1 continued

Year	Ln INCOME					COEFFICIENT OF VARIATION		ATKINSON'S MEASURE	
	White mean	White s.d.	Non-white mean	Non-white s.d.	Diff.	White	Non-white	White	Non-white
1949	7.867	1.022	7.240	.922	.627	.852	.924	.199	.191
1950	7.955	1.029	7.335	1.026	.620	.842	.856	.198	.200
1951	8.012	.974	7.429	.909	.583	.758	.746	.181	.170
1952	8.061	.960	7.471	.931	.590	.922	.998	.189	.184
1953	8.045	1.055	7.452	.979	.593	.875	.765	.205	.191
1954	8.048	1.025	7.365	1.082	.683	.882	.998	.206	.226
1955	8.087	1.040	7.459	1.044	.628	.910	.792	.210	.211
1956	8.141	1.061	7.488	1.061	.653	.900	.823	.213	.216
1957	8.110	1.073	7.473	1.090	.637	.863	.821	.213	.224
1958	8.094	1.118	7.441	1.120	.653	.864	1.021	.222	.243
1959	8.166	1.096	7.474	1.100	.692	.880	1.006	.219	.246
1960	8.155	1.136	7.543	1.126	.612	.899	.874	.230	.245
1961	8.170	1.176	7.569	1.105	.601	.906	.930	.239	.242
1962	8.206	1.150	7.592	1.082	.614	.863	.819	.229	.225
1963	8.217	1.152	7.637	1.146	.580	.852	.847	.229	.238
1964	8.233	1.176	7.727	1.102	.506	.858	.959	.235	.240
1965	8.258	1.178	7.697	1.098	.561	.863	.824	.235	.231
1966	8.320	1.146	7.781	1.096	.539	.841	.802	.229	.226
1967	8.341	1.154	7.789	1.150	.552	.835	.848	.229	.238
1968	8.365	1.168	7.838	1.108	.527	.815	.807	.227	.233
1969	8.383	1.185	7.871	1.119	.512	.807	.791	.230	.227
1970	8.359	1.214	7.874	1.144	.485	.815	.813	.234	.234
1971	8.360	1.220	7.852	1.195	.508	.807	.831	.233	.241
1972	8.420	1.208	7.937	1.154	.483	.794	.827	.229	.237
1973	8.428	1.226	7.957	1.160	.471	.774	.812	.228	.236

NOTE: These estimates are based on grouped income data published in Current Population Reports, series P-60. In order to minimize the effects of grouping, Schwartz wrote a program which estimated each category's mean and variance on the assumption that each income category was a segment of a normal distribution. Economists would doubtless prefer to assume a log-normal or Pareto distribution but such changes would not appreciably alter the results shown here.

change in the year-to-year change. The Race x Year interaction measures the average change in the difference between whites and non-whites, and the Race x Year^2 interaction measures the average change in the trend of the difference between the two groups. Because the units of analysis are income distributions rather than individuals (although each distribution is based on a sample of several thousand individuals) the standard errors and R^2's should be interpreted with caution. Because all five independent variables have been made mutually orthogonal,[2] the correlations of the independent with the dependent variables equal the standardized regression coefficients. This also means that any variable may be removed from the equations without affecting the remaining regression coefficients.

We begin our examination of Table 8.2 by giving a detailed interpretation of the Income equation , and proceed to discuss interesting points in the other equations. We first observe that the mean income of the total population (in 1967 dollars) increased by an average of $136 per year. This increase is essentially linear, although the positive coefficient of Year^2 does indicate that the increase grows slightly from year to year. Over this 25-year period white income averaged $2435 higher than non-white income. This difference increased at an average rate of $37 each year. However, the negative but small coefficient of Race x Year^2 indicates that the rate of increase was shrinking slightly. So although the gap between white and non-white income widened every year, it widened by a smaller amount. The large R^2 of .993 represents the percentage of the variance in the means explained by these independent variables.

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[2] See coding at bottom of Table 8.2.

Table 8.2

REGRESSIONS OF INCOME, INCOME TRANSFORMATIONS AND MEASURES OF INEQUALITY ON RACE AND YEAR (CPS-1967 DOLLARS)

DEPENDENT VARIABLE			RACE	YEAR	$YEAR^2$	RACE x YEAR	RACE x $YEAR^2$	constant	R^2	Mean	s.d.
1)	$\overline{Y}$	B	2435.16	135.55	[.14]	37.13	[-1.8[illegible]]	-5146.27	.993	5321	1240
		s.e.	(52.96)	(2.20)	(.34)	(7.34)	(1.14)				
		r	.595	.796	.005	.065	-.021				
2)	Sy	B	2038.20	96.91	-2.89	[22.17]	[-3.32]	-3084.25	.971	4511	964
		s.e.	(81.33)	(3.38)	(.33)	(11.28)	(1.75)				
		r	.641	.732	-.140	.050	-.048				
3)	$\overline{Y^{1/3}}$	B	2.896	.127	[-.000]	[-.010]	[-.002]	5.626	.989	15.957	1.282
		s.e.	(.068)	(.003)	(.000)	(.009)	(.001)				
		r	.685	.720	-.006	-.017	-.027				
4)	$S_y{}^{1/3}$	B	.790	.062	-.001	[.007]	[.000]	.644	.969	5.076	.524
		s.e.	(.046)	(.002)	(.000)	(.006)	(.001)				
		r	.456	.863	-.123	.029	.001				
5)	$\overline{\text{LnY}}$	B	.580	.021	[-.000]	-.007	[-.001]	6.329	.983	8.134	.236
		s.e.	(.015)	(.001)	(.000)	(.002)	(.000)				
		r	.745	.650	-.009	-.063	-.032				
)	S_{LnY}	B	.033	.010	-.000	[.001]	[.000]	.492	.884	1.114	.078
		s.e.	(.013)	(.001)	(.000)	(.002)	(.000)				
		r	.129	.921	-.127	.035	.043				
7)	10V	B	[-.103]	-.029	-.005	[.001]	[-.001]	10.668	.461	8.521	.475
		s.e.	(.174)	(.007)	(.001)	(.024)	(.004)				
		r	-.066	-.450	-.502	.004	-.040				
8)	100A	B	[-.399]	.180	-.014	[-.027]	[.007]	12.133	.832	22.016	1.622
		s.e.	(.331)	(.014)	(.002)	(.046)	(.007)				
		r	-.075	.808	-.410	-.037	.062				
Coding			0-non-white 1-white	49-73	$(YEAR-61)^2$	(Race-.9) x (Year-61)	(Race-.9) x ($Year^2$-52)				
mean			.900	61.000	52.000	0	0				
s.d.			.303	7.284	46.869	2.185	14.061				

Note: In all tables, coefficients in brackets are less than twice their standard error.

In contrast to the first equation which measures income in dollars, the equation for Ln Income ($\overline{\text{LnY}}$) indicates that whites earned an average of $e^{.580}$=1.79 times as much as non-whites, but that this percentage advantage decreased by an average of $e^{.007}$=0.007 each year, or (25)(0.007) = 0.175 over 12 years. Using Income$^{1/3}$, the annual decrease in the gap was very small. In fact, since the contribution of each variable to R^2 equals r^2 (because of the mutual orthogonality of the independent variables), and the combined contribution of Year^2, Race x Year and Race x Year^2 is only .001, we essentially have a 2-variable equation in which both the white and non-white cube root means rose every year but the absolute gap between them remained virtually constant.

We now turn our attention to an examination of three scale-invariant measures of within-race inequality: the standard deviation of Ln Income (S_{ln}), the coefficient of variation (V) and Atkinson's measure of inequality (A). V measures inequality in such a way as to place primary emphasis on the ratio of top incomes to the mean, while S_{ln} places primary emphasis on the ratio of low incomes to the mean. The two measures can therefore yield contrary impressions of the trend in inequality. Atkinson's measure (A) is normative instead of simply descriptive (see Sen). It tries to weight incomes according to their contribution to overall well-being or utility. If one uses Income$^{1/3}$ as the measure of utility, as we do, results using A will usually fall between those using V and those using S_{ln}.

Looking first at trends over time, Equation 7 shows that inequality (as measured by V) within both racial groups declined from 1949 to 1973. But Equations 6 and 8 show that if we look at S_{ln} or A, inequality increased. This suggests that those at both the top and the bottom of the income distribution lost ground relative to those in the middle. This conclusion must be treated cautiously, however. First, it only covers income recipients, ignoring possible changes in the percentage of men 25-64 without income. Second, it is based on grouped data and may be sensitive to changes in the ratio of the category cut-off points to the mean.[3/]

Turning to inequality within racial groups, we see that there is no consistent difference between whites and non-whites on two of our three measures of inequality (V and A). Whites appear slightly less equal than non-whites on the third index (S_{1n}). This result is in some sense a by-product of the fact that whites can be further below the mean than non-whites without having negative incomes.

Finally, note that the value of R^2 is unusually low for V. This reflects large year-to-year fluctuations in V for both non-whites and whites, especially prior to 1960. These fluctuations in V are due to fluctuations in the small percentage of respondents in the top two income categories. While such fluctuations are probably due to random sampling error and rounding to the nearest tenth of a percent, they can seriously affect our estimate of S_Y.

[3/] If the true shape of the distribution is stable while the mean rises, if the cut-off points for the categories (e.g., $15,000 to $24,999) remain constant, and if one assigns every individual in a category the mean of the category, V will fall while S_{1n} rises. Our procedure for calculating within-category variances may not entirely eliminate this potential bias, since it understates the variance in the extreme categories of the distribution.

[4/] The lowest CPS category falls further below the white mean than below the non-white mean. Whites can therefore have lower values relative to the white mean than non-whites can have relative to the non-white mean.

2. Comparing CPS to Other Samples

Up to this point we have used the CPS to investigate time-trends in white and non-white income distributions. In later sections we intend to examine the extent to which white/non-white differences in income relate to differences in background, education, and experience. One can investigate the effects of race and education on earnings using annual CPS data from the 1960's (see Winsborough 1975).

As CPS publications do not provide information on background variables we must turn to our other national samples. Before examining multivariate regressions from these samples, however, it is important to investigate whether they are really comparable, because their target populations clearly differ in several respects. OCG, for example, includes students, members of the military, and men without earnings who had income from other sources. Our PA, NLS, Census, and PSID samples exclude such men. OCG, NLS, and Census also include men who are not household heads, while PA and PSID exclude them. Finally, OCG uses grouped income data to measure economic success, whereas the other four surveys use an ungrouped earnings measure. McClelland finds that these differences have relatively little impact on the relationship between education and earnings (see Chapter 16), but he does not look at their impact on racial differences. We therefore compared each of our five samples to the CPS sample for the same year. Assuming that differences between CPS samples reflect secular trends, this procedure should help clarify how our five samples differ from one another.

Table 8.3 compares the income or earnings distribution from each sample with the income distribution from the Current Population Survey for the corresponding year. (As noted in Chapter 16, the difference between earnings and total reported income is very small for men in this age group.) We computed both goodness-of-fit and maximum-likelihood χ^2 statistics (Bishop, et al., 1975, pp. 57-8) to see how much each sample differed from the CPS sample for the same year. The NLS and the OCG distributions do not significantly differ from the corresponding CPS distributions, but the other three samples do.

In order to say which samples differ most from the CPS yardstick, we recall that χ^2 depends on both the sample sizes and the magnitude of the difference between two samples. To obtain an estimate of the magnitude of the difference between two distributions that is independent of the sample size, we computed a "standardized" χ^2 that assumed a sample size of 10,000.

Table 8.3

COMPARISON OF EARNINGS AND INCOME DISTRIBUTIONS IN OUR SAMPLES WITH CORRESPONDING CPS INCOME DISTRIBUTIONS

Distributions

	1961		1964		1966		1969		1971	
	CPS (Inc)	OCG (Inc)	CPS (Inc)	PA (Earn)	CPS^{a} (Inc)	NLS^{a} (Earn)	CPS (Inc)	CENSUS (Earn)	CPS (Inc)	PSID (Earn)
$1-999	6.5	6.7	4.9	2.1	3.5	3.0	2.5	2.3	2.3	1.9
1000-1999	6.5	6.4	5.9	4.1	3.7	3.7	3.3	2.9	3.1	1.9
2000-2999	8.5	8.2	6.4	4.9	5.7	4.4	3.7	3.1	3.6	1.9
3000-3999	11.0	11.3	9.5	7.3	6.8	6.6	4.8	4.7	4.3	3.1
4000-4999	13.6	13.3	11.0	8.0	7.6	7.8	6.0	5.6	5.3	4.8
5000-5999	15.6	15.4	13.2	13.1	11.6	11.3	7.7	7.8	6.2	4.9
6000-6999	11.8	11.9	13.0	13.0	12.2	12.3	9.0	9.3	7.2	5.8
7000-7999	8.9	9.4	10.7	13.1	11.4	11.1	10.6	10.8	9.0	8.6
8000-9999	7.9	7.8	11.4	15.3	15.1	16.5	18.1	18.6	16.7	14.8
10,000-14,999	6.4	6.4	9.8	13.1	15.2	16.1	23.1	23.2	27.1	31.0
15,000-24,999	2.5	2.4	3.3	4.1	5.1	5.3	8.7	8.6	11.7	16.4
25,000 +	.9	.9	.8	2.1	2.2	2.0	2.5	3.1	3.5	5.1
N^{b}	17140	11505	16750	1188	6050	2831	32300	38714	31350	1774

Means and Standard Deviations (1967 dollars)^{c}

Income										
mean	6507	6497	7031	8376	8168	8310	8427	8653	8358	9492
s.d.	4972	4968	4820	5871	5815	5610	5294	5531	5226	5589
Income 1/3										
mean	17.627	17.611	18.194	19.416	19.125	19.313	19.436	19.623	19.361	20.287
s.d.	4.537	4.570	4.364	4.273	4.604	4.416	4.347	4.369	4.397	4.413
Ln Income										
mean	8.508	8.504	8.611	8.824	8.761	8.798	8.817	8.849	8.802	8.949
s.d.	.852	.863	.801	.695	.799	.757	.753	.743	.767	.748

t-Tests of Differences Between Me[illegible]

Income					
Difference	-10	1345	142	226	1134
t	-0.17	7.71***	1.10	5.55***	8.34***
Income 1/3					
Difference	-0.16	1.222	.188	.187	.926
t	-0.29	9.86***	1.84	5.70***	8.60***
Ln Income					
Difference	-.004	.213	.037	.032	.147
t	-0.39	10.10***	2.11*	5.67 ***	8.04***

χ^2 Analysis (11 degrees of freedom)

Goodness of Fit χ^2	4.773	99.040***	12.096	63.838***	94.868 ***
Standardized ^{d}	(1.666)	(55.256)	(13.606)	(8.989)	(28.637)
Maximum Likelihood χ^2	4.769	97.909***	12.303	63.909 ***	95.480 ***
Standardized ^{d}	(1.664)	(54.625)	(13.839)	(8.999)	(28.822)

Notes for Table 8.3

a. The NLS is restricted to men aged 45-59. The 1966 CPS data include only men aged 45-54. CPS does not present data on men 55-59 separately. We considered adding a portion of the CPS distribution for men aged 55-64 to the distribution for men aged 45-54 in order to approximate a distribution for men aged 45-59. However, since Census data indicate that the income distribution of men aged 55-59 is more similar to that of men aged 45-54 than it is to that of men aged 55-64 (due partially to the effects of retirement and semi-retirement among men aged 60-64), we decided against this.

b. The N's for CPS are estimated from published data on the total CPS sample size.

c. Means and standard deviations were obtained using procedures described in the note for Table 1.

d. The standardized χ^2 assumes that the combined total sample size is 10,000. Operationally, it equals $10{,}000\chi^2/N$.

* p 0.05

*** p 0.001

These values are shown in parentheses. It is clear that the Census, OCG and NLS samples differ from the corresponding CPS samples much less than do the PSID and PA samples. This pattern is not hard to explain. The OCG and the NLS samples were based on the 1962 and 1967 CPS surveys (see Appendices B and F). The Census is carried out by the same organization, but uses different procedures. The PA and PSID surveys were conducted by the Survey Research Center (SRC) at Michigan, an unrelated organization.

Table 8.3 also presents the means and standard deviations of annual Earnings, Earnings$^{1/3}$, and Ln Earnings along with t-statistics testing the hypothesis that the means of the five samples equal the means of the corresponding CPS samples. These statistics merely confirm what our earlier analysis indicated: the OCG and NLS means are not significantly different from the corresponding CPS means,[5] the Census exhibits a significant but relatively small difference, while the PA and PSID means each differ from the corresponding CPS means by more than $1,000. The difference between CPS and our other samples is partly due to different sample restrictions. However, McClelland shows in Chapter 16 that SRC respondents report higher earnings than Census respondents even when sample restrictions are identical, and Jackson shows in Chapter 15 that they more often report holding white-collar jobs.

Table 8.4 shows the education distribution for the five national samples and for four corresponding CPS samples.[6] Once again the χ^2

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[5] The NLS mean of Ln Earnings is just significantly greater than the 1966 CPS mean. This is because the two distributions differ more at the lower end than at the upper end.

[6] No CPS/ education distribution is available for 1966 income recipients aged 45-59 in 1967.

Table 8.4

COMPARISONS OF : EDUCATION DISTRIBUTIONS IN OUR SAMPLES WITH CORRESPONDING CPS DISTRIBUTIONS

Years of Education	1962[a]		1965[a]		1967		1970		1972	
	CPS	OCG	CPS	PA	CPS	NLS	CPS	CENSUS	CPS	PSID
8 or less	32.3	30.7	29.0	25.0	NA	37.4	21.9	21.2	19.6	19.0
9-11	18.5	19.0	18.2	19.4	NA	19.9	16.6	19.2	16.6	16.8
12	27.2	27.6	30.2	29.9	NA	24.4	33.8	31.7	34.4	31.0
13-15	9.9	10.0	9.6	11.4	NA	8.5	12.0	12.0	12.5	15.8
16	7.1	7.5	7.4	10.9	NA	5.0	8.6	7.9	9.1	11.0
17 +	5.1	5.4	5.5	3.4	NA	4.8	7.0	8.1	7.8	6.4
N[b]	17140	11505	16750	1188		2831	32300	38714	31350	1774
mean	10.643	10.758	10.858	11.105		10.203	11.467	11.493	11.672	11.759
s.d.	3.468	3.467	3.429	3.312		3.428	3.375	3.402	3.354	3.326
difference	.115		.247				.026		.087	
t	2.756 *		2.479 *				1.001		1.071	

χ^2 Analysis (5 degrees of freedom)

	1962	1965	1970	1972
Goodness of Fit χ^2	9.650	37.679 ***	132.345 ***	31.292 ***
standardized[c]	(3.366)	(21.025)	(18.635)	(9.447)
Maximum Likelihood χ^2	9.659	37.104 ***	132.746 ***	30.356 ***
standardized[c]	(3.370)	(20.704)	(18.691)	(9.164)

a/ CPS education distributions for men with income in years prior to 1967 were available only for men aged 25 and over, and thus included men aged 65 and over. To make the 1962 and 1965 CPS education distributions more comparable with the OCG and PA distributions, we assumed that men over 65 in 1962 and 1965 had the same education distribution as men over 65 in the 1970 CPS sample. Because men 65 and over in 19 were probably better educated than men of the same age in 1962 and 1965, this adjustment does not eliminate enough poorly-educated men. The resulting CPS distributions are therefore likely to display a small downward bias.

b/ The N's for CPS are estimated from published data on the total CPS sample size.

c/ Standardized $\chi^2 = 10{,}000\ \chi^2/N$.

* p .05
** p .01
*** p .001

statistics show that the OCG does not differ significantly from the corresponding CPS. But contrary to expectations, the Census distribution is quite different from the CPS. Indeed, the standardized Census-CPS χ^2 is larger than for PSID-CPS and almost as large as for PA-CPS. Yet despite these large χ^2 statistics, the t-tests indicate that the Census and PSID means do not differ from the means of the corresponding CPS education distributions any more than we would expect by chance. The PA and OCG means are significantly higher than the CPS means, but this could be because our adjustment of pre-1967 CPS education distributions (see Table 8.4, note a) overestimated the attainment of men 65 and over. Thus there is no strong evidence that OCG, PA, Census, or PSID finds more highly- or poorly-educated men than CPS. The differences in χ^2 are largely due to differences in the shapes of the distributions, not differences in central tendency. The form of the questions, the way in which the questionnaires were administered, or the way in which they were coded could all contribute to this. SRC surveys, for example, reserve the categories 12, 16 and 17 + for those who have received diplomas or degrees, while Census surveys merely count years of education completed.

. Also, SRC assigns non-respondents on the basis of literacy, while CPS uses a "hot deck" routine described in Appendix A. McClelland discusses these issues in more detail in Chapter 16. Bishop (1974) also discusses possible reasons for the difference between Census and CPS education distributions. Allowing credentials and literacy to influence the coding of Years of Education can affect the regression coefficients in earnings equations.

Finally, Table 8.5 compares the percentages of whites and non-whites in our samples to those of the CPS. With the exception of the PSID the distributions are similar. The difference between the PSID sample and the 1971 CPS is presumably due to Mueser's classification of respondents with Spanish surnames as non-white in the survey. We do not know to what extent this makes PSID results for non-whites different from results for non-whites in other surveys.

3. Determinants of Education

This and the following section will discuss regression equations predicting educational attainment and annual earnings in the PSID, OCG and Census samples. Table 8.6 shows the means and standard deviations of the relevant variables for these three samples. We will not consider the NLS, since it includes only men aged 45-59, nor the PA, since it includes only 137 non-whites. The Census will be considered only when our analyses do not require background data. We will not discuss the determinants of occupational status Those with a particular interest in this outcome will find the relevant regressions at the end of the chapter. Olneck also discusses white-nonwhite differences in the effects of education on occupational status in Chapter 6.

We subtracted the total sample mean from each independent variable before running our regressions. This has no effect on the coefficients or their standard errors, but makes the constant equal to the expected value of the dependent variable for an individual who is at the total sample mean on all the independent variables. Thus the difference

Table 8.5

COMPARISONS OF RACE DISTRIBUTIONS IN OUR SAMPLES WITH CORRESPONDING CPS DISTRIBUTIONS

Race	1962		1965		1967		1970		1972	
	CPS	OCG	CPS	PA	CPS	NLS	CPS	CENSUS	CPS	PSID
Non-white	9.7	9.6	9.6	9.9	NA	9.1	10.0	9.9	10.0	12.0
White	90.3	90.4	90.4	90.1	NA	90.9	90.0	90.1	90.0	88.0
N[a/]	17140	11505	16750	1188		2831	32300	38714	31350	1774

χ^2 Analysis (1 degree of freedom)

	1962	1965	1967	1970	1972
Goodness of Fit χ^2	0.079	0.115		0.197	7.392**

a/ CPS N's are estimated from published data on the total CPS sample size.

** Significant at the 0.01 level

Table 8.6

MEANS AND STANDARD DEVIATIONS BY RACE OF MEN AGED 25-64 WITH COMPLETE DATA AND POSITIVE EARNINGS

	CENSUS 1970		OCG 1962		PSID 1972	
	White (N=28.615)	Non-White (N=2082)	White (N=10.395)	Non-White (N=1110)	White (N=1260)	Non-White (N=514)
Age	43.0317 (10.9981)	41.8353 11.0008	42.6942 (10.7000)	41.4483 (10.7700)	42.5837 (10.9328)	41.6344 (10.8438)
Age^2	1972.6819 (963.6153)	1871.3463 (953.5610)	1773.1709 (111.3521)	1768.2934 (113.0313)	1766.9730 (119.4679)	1766.5167 (113.2122)
Father's Education			7.9130 (3.8813)	5.9951 (3.8593)	8.6907 (3.1515)	7.6385 (3.3191)
Father's Occupation			28.4151 (20.9359)	18.7675 (13.2086)	28.8650 (17.9769)	24.1461 (18.6576)
Father White Collar			.2552 (.4266)	.1095 (.2645)	.2371 (.4242)	.1668 (.3663)
Father Absent			.1393 (.3462)	.3113 (.4632)	.0060 (.0771)	.0284 (.1664)
Non-South Upbringing	.7340 (.4419)	.3112 (.4631)	.7499 (.4331)	.3012 (.4590)	.7394 (.4392)	.3418 (748)
Non-Farm Upbringing			.7919 (.4059)	.7692 (.4215)	.6929 (.4615)	.6217 (.4854)
Siblings			4.2226 (3.0446)	5.6237 (3.7292)	3.6873 (2.6301)	5.4105 (2.8594)
Father's Education^2			-48.8545 (21.9032)	-45.0428 (22.7574)	-82.7809 (15.0184)	-78.5230 (18.1390)
Father's Occupation^2			-1010.0595 (472.1927)	-963.3397 (396.8545)	-1119.5305 (337.1655)	-973.1416 (319.7982)
Siblings^2			-21.2467 (11.9206)	-18.9337 (18.0827)	-14.0433 (7.0793)	-13.2649 (7.7063)
Years of Education	11.7489 (3.4274)	9.8876 (3.9817)	11.1783 (3.5517)	8.5510 (4.0930)	12.1900 (3.1720)	9.9631 (3.9318)
Years of Education Past High School	1.0952 (2.0297)	.5985 (1.5860)	.9472 (1.7979)	.3524 (1.0922)	1.2750 (1.8958)	.6405 (1.5634)
BA	.1691 (.3748)	.0859 (.2799)	.1500 (.3571)	.0489 (.2157)	.1950 (.3963)	.0961 (.2896)
Experience	24.1529 (12.0418)	24.3429 (12.3628)	24.2786 (11.8586)	24.0203 (11.8395)	23.3214 (11.9019)	24.0966 (12.0007)
Experience^2	728.3612 (615.4593)	745.3439 (650.5222)	-494.4929 (144.3176	-495.7846 (141.0061)	-466.1512 (146.1732)	-465.4777 (143.9479)
Occupation	42.0578 (24.4658)	26.4659 (20.5437)	39.9984 (24.7421)	20.3243 (18.1615)	42.3902 (20.8736)	29.2636 (18.9328)

Table 8.6 continued

	Census 1970		OCG 1962		PSID 1972	
	White (N=28615)	Non-White (N=2082)	White (N=10,395)	Non-White (N=1110)	White (N=1260)	Non-White (N=514)
Earnings (1967 dollars)	9227.1148 (7124.5058)	5895.3140 (4188.3591)	7930.5095 (4862.4950)	3545.4465 (2527.3826)	9843.7066 (6266.2245)	6777.4254 (6308.0983)
Earnings $^{1/3}$	20.9551 (4.2458)	17.2258 (3.9159)	8.2770 4.1094)	14.3960 (3.8550)	20.5892 (4.5346)	17.8243 (4.2327)
Ln Earnings	8.9267 .6949	8.4458 .8023	8.6289 .7685	7.8509 .9374	9.0024 .7003	8.5128 1.0009
Sentence Completion Test Score					10.1358 1.7796	8.4415 2.6050

between the constants for the white and non-white equations is the expected difference between whites and non-whites who are at the **overall** mean on all the independent variables.

Social Background and Education

Table 8.7 presents two equations predicting educational attainment. Equation 1 shows the effects of Age **and demonstrates an accelerating** tendency for successive cohorts to obtain more education. This trend is stronger for non-whites than for whites (significantly so in the OCG and PSID samples).[7] Controlling age, the difference between white and non-white educational attainment (i.e., the difference between the constants) is larger than the observed differences. This is because non-whites are younger than whites[8] and young men ordinarily **obtain more education than their elders did.**

Equation 2 includes family **background variables for the OCG** and PSID samples. Except for Father's Occupation, none of the coefficients in the white OCG and PSID equations differ significantly.[9] At first sight this seems to indicate a remarkably stable relationship between two surveys conducted a decade apart. **However,** about 3/4 of all men 25-64 in 1962 were still 25-64 in 1972, so one would not expect much change even if the determinants of young men's educational attainment differed from those of their elders. Still, the similarity indicates a reassuring degree of comparability **between the two white samples.**

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[7] The criterion for significance employed throughout this section is that the difference between two coefficients should be greater than twice the square root of the sum of their v squared standard errors.

[8] See Table 8,6,

[9] **This may be due to the fact that OCG and PSID measured Father's Occupation differently.**

Table 8.7

REGRESSIONS OF EDUCATION ON AGE AND BACKGROUND CHARACTERISTICS OF MEN AGED 25-64 WITH COMPLETE DATA AND POSITIVE EARNINGS, BY RACE

		Census 1970		OCG 1962		PSID 1972	
		White (N=23,615)	Non-white (N=2082)	White (N=10,395)	Non-white (N=1110)	White (N-1260)	Non-white (N=514)
Equation 1							
AGE	B	[.0506]a/	[.0044]a/	-.0816 **	-.1292	-.0614 **	-.1163
	s.e.	(.0167)	(.0611)	(.0032)	(.0108)	(.0080)	(.0152)
AGE^2	B	-.0013a/	-.0016a/	-.0008	[-.0006]	-.0015	[-.0000]
	s.e.	(.0002)	(.0007)	(.0003)	(.0010)	(.0007)	(.0015)
Constant		11.7548	9.7472	11.1877	9.4084	12.1960	9.8643
R^2		.0447	.1308	.0613	.1144	.0481	.1029
S.D. of Resid.		3.3500	3.7130	3.4414	3.8553	3.0972	3.7314
Equation 2							
AGE	B			-.0556 **	-.0959	-.0364 **	-.0931
	s.e.			(.0027)	(.0099)	(.0073)	(.0133)
AGE^2	B			-.0013	[-.0014]	-.0015	-.0030
	s.e.			(.0003)	(.0009)	(.0006)	(.0013)
Father's Education	B s.e.			.2015 (.0086)	.2395 (.0332)	[.2172] (.0286)	.2780 (.0564)
Father's Occupation	B s.e.			.0248 (.0023)	[.0147] (.0136)	[-.0001] (.0098)	.0412 (.0186)
Father White Collar	B s.e.			.9120 (.1087)	[.8664] (.6091)	1.3229 (.3774)	[1.4184] (.9201)
Father Absent	B s.e.			-.8940 * (.0839)	-1.5492 (.2442)	[-.6540] * (1.0020)	-4.0805 (.8622)
Non-South Upbringing	B s.e.			.6683 (.0680)	1.1384 (.2270)	.7351 (.1813)	[.1250] (.3564)
Non-Farm Upbringing	B s.e.			.2959 ** (.0760)	1.9657 (.2540)	[.3154] (.1984)	.7448 (.3147)
Siblings	B			-.1975 **	-.0903	-.2562 *	[-.0282]
	s.e.			(.0101)	(.0322)	(.0312)	(.0562)
Father's $Education^2$	B s.e.			[.0011] (.0014)	[-.0017] (.0052)	[-.0016] (.0055)	[-.0084] (.0088)
Father's $Occupation^2$	B s.e.			[-.0001] (.0001)	-.0007 (.0003)	-.0005 (.0002)	[-.0009] (.0005)
$Siblings^2$	B			.0126 **	[-.0076]	[.0030] *	.0615
	s.e.			(.0024)	(.0062)	(.0110)	(.0203)
Constant				11.0428	10.0132	12.0702	10.7339
R^2				.3250	.3326	.2691	.4010
S.D. of Resid.				2.9197	3.3594	2.7248	3.0793

Percentage of gap eliminated if equalization occurs at level of:

	OCG	PSID
Non-white means	53.4	51.1
White means	61.6	38.7

Notes on Table 8.7

* The ratio of the difference between whites and non-whites to the standard error of the difference is greater than 2 but less than 3.

** The ratio of the difference between whites and non-whites to the standard error of the difference is equal to or greater than 3.

a/ Age^2 was not made orthogonal to Age in the Census regressions. All other squared terms were orthogonalized (See Chapter 17).

There are several differences between the OCG and PSID non-white equations. The negative relationship between not having had a father at home and educational attainment appears stronger among PSID non-whites than among OCG non-whites. This is probably because the Father Absent variable in the PSID is based on failure to report any of one's father's characteristics, not on actual data about whether the father was present. Having grown up in the South or on a farm has a smaller negative effect on PSID non-whites than on OCG non-whites. The two samples of non-whites may be less comparable than the two samples of whites because non-whites born between 1938 and 1947 differ more from their predecessors than whites born between 1938 and 1947. In addition, inclusion of respondents with Spanish surnames in the PSID non-white sample may contribute to differences. Alternatively, the difference may be due to the smaller effective size of the non-white subsamples or to changes in the quality of the non-white data.

OCG Equation 1 shows that an extra year of age implies an average of 0.056 years less education for whites and 0.096 years less for non-whites. The difference between these coefficients, -0.040, indicates that the gap between whites and non-whites is decreasing at the rate of 0.04 years of education a year. A 30-year-old non-white is thus about 1 year closer to his white counterpart in educational attainment than a 55-year-old non-white, since (55-30)(.04)=1.00. PSID Equation 1 implies a similar pattern.

The lack of a father affects a non-white's education more than a white's. In the OCG sample, whites who grew up without a father have an average of 0.894 years of education less than similar whites who grew up with fathers. Fatherless non-whites have an average of 1.549 years less education than non-whites with a father, all other things being equal. The gap between the educational attainment of whites and non-whites is thus 1.549-0.894=0.655 years greater for those who grew up without a father. Unfortunately, we cannot confirm this conclusion using PSID data, since the PSID measure of father absence is so problematic.

Growing up in a small family is more advantageous to whites than to non-whites.[10] The interaction of Siblings^2 with Race in OCG is reversed in PSID. This may reflect the fact that PSID groups respondents with 8 or more siblings, while OCG does not. We can see no obvious substantive implication.

The effect of Nonfarm Upbringing is significantly different for whites and non-whites in the OCG sample, but not in the PSID sample. In the OCG sample the education gap is 1.67 years greater for whites at the rate of 0.04 years of education a year. A 30-year-old non-white is thus about 1 year closer to his white counterpart in educational attainment than a 55-year-old non-white, since (55-30)(.04)=1.00. PSID Equation 1 implies a similar pattern.

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[10] A linear coefficient should usually be interpreted with caution when the squared term Siblings^2 is also included, as in Equation 2. However, the squared terms in the OCG and PSID were orthogonalized for the total sample. While they are not exactly orthogonal within each race, the correlations are low. Inclusion of the squared terms therefore has a negligible effect on the coefficient of the corresponding linear term. When we omitted the squared term, the results were not appreciably altered.

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The effect of Nonfarm Upbringing is significantly different for whites and non-whites in the OCG sample, but not in the PSID sample. In the OCG sample the education gap is 1.67 years greater for whites

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[10/] A linear coefficient should usually be interpreted with caution when the squared term Siblings^2 is also included, as in Equation 2. However, the squared terms in the OCG and PSID were orthogonalized for the total sample. While they are not exactly orthogonal within each race, the correlations are low. Inclusion of the squared terms therefore has a negligible effect on the coefficient of the corresponding linear term. When we omitted the squared term, the results were not appreciably altered.

and non-whites who grew up on farms than for those who did not. Non-South Upbringing has a similar but not quite significant effect in the OCG.

These findings are in close agreement with those based on the 1973 OCG-II as reported by Hauser and Featherman (1976). They concluded that farm background is a greater disadvantage to non-whites than to whites, although its effect on both groups' educational attainment has been decreasing with time. They also show that a large number of siblings is more of a disadvantage to -whites than to non-whites. Finally, their data indicate that the differences in education between whites and non-whites decreases in younger cohorts.

We now examine to what degree the educational gap between whites and non-whites is dependent on differences in background. Non-whites typically come from less advantaged backgrounds than whites. They more frequeutly grow up in broken homes. If there is a father at home he does not usually have as much education as the average white father. Non-whites are also more likely than whites to grow up in the South, on farms, and in large families. All these factors tend to have a negative effect on their educational attainment. If the average non-white's background were the same as the average white's background (i.e. if non-white means on background variables were equal to white means), the expected non-white mean education would be 10.171 years for the OCG sample rather than the observed mean of 8.551 years, a difference of 1.620 years. The gap between white and non-white average educational attainment (2.627 years) would thus be decreased by 61.6 per cent. The largest part of this change is due to the effects of three variables: Father's Education, Father Absent and Non-South Upbringing. If non-white means on these variables alone were raised to the white means, the educational gap would decrease by 47.1 per cent.

The effect of background on education varies according to the "level" of background chosen as the reference point. The average OCG white had 2.627 years more education than the average non-white. The gap in educational attainment between the average OCG white and OCG non-whites with the same background as the average white was (1-0.616) (2.627) = 1.009 years. In contrast, the white/non-white gap for those with the same background as the average OCG non-white was (1-0.534) (2.627) = 1.219 years. This means that in 1962 the educational gap between whites and non-whites was smaller among those from more advantaged backgrounds. The PSID data imply that this pattern changed between 1962 and 1972. The overall difference between white and non-white educational attainment fell from 2.627 to 2.227 years. This appears to have been largely due to a decrease in background differences during the interval. The greatest gainers appear to have been disadvantaged non-whites. If we compare the average non-white to a white from a similar background, the white's advantage only fell from 1.219 to 1.089 years. If we compare the average white to a non-white from a similar background the white's estimated advantage rose from 1.009 to 1.365 years. This latter figure presumably has a large sampling error, since there are not many PSID nonwhites from backgrounds similar to that of the average white. Still, it seems safe to conclude that in both 1962 and 1972 whites aged 25-64 had between 1.0 and 1.4 years more schooling than non-whites from similar demographic backgrounds.

Part of this difference in educational attainment is undoubtedly traceable to differences in cognitive skills prior to school completion. Our data do not, however, provide a reliable basis for determining how much of the gap such differences explain.

4. Determinants of Earnings

Table 8.8 presents regressions of Earnings, Earnings$^{1/3}$ and Ln Earnings on Education and Experience for the OCG, Census and PSID samples. The highest R^2's are produced by the cube root transformation and by the PSID sample. The OCG calculations are based on a categorized income variable. McClelland concludes in Chapter 16 that this

has no predictable effect on either regression coefficients or R^2. Nevertheless, when dealing with grouped data, the size of the top and bottom income categories and the values assigned them can substantially affect regression results.

We begin by discussing the Ln Earnings equations. The white coefficients for Years of Education are similar in these samples and indicate an increase of about 8 percent in earnings for each year of schooling. In none of the equations is the non-white coefficient for Years of Education significantly different from the white coefficient. Nevertheless, the non-white coefficient decreases significantly between the 1962 OCG and the 1972 PSID.

The coefficient of BA represents the extra percentage increase in earnings (over and above the average return to a year of education) for completing the fourth year of college. In the OCG white and non-white samples college graduates earned an insignificant amount more than one would expect if returns to schooling were uniform at all levels. (Actually, returns are higher for BA's, but this is offset by low returns for college dropouts and those with graduate education - see Appendix B.) In the Census and PSID white samples, the last year of college is worth 2 to 3 times as much as other years of schooling. This may reflect either time trends, different sample restrictions, or differences in the dependent variable.

The pattern for non-whites is **more puzzling. BA has an insignificant coefficient for the non-white samples in both the OCG and Census, but an exceptionally large positive coefficient in the PSID. One hypothesis is that the value of the last year of college rose from 1969 to 1971 because of "affirmative action policies." To test this hypothesis we examined the PSID subsample of male non-student, non-military heads of households aged 25 or over in 1967 and 65 or under in 1972. We computed white and non-white equations analogous to those in Table 8.8 for each year from 1967**

Table 3.8

REGRESSIONS OF EARNINGS, EARNINGS $^{1/3}$ AND LN EARNINGS ON EDUCATION AND EXPERIENCE OF MEN AGED 25-64 WITH COMPLETE DATA AND POSITIVE EARNINGS, BY RACE (1967 DOLLARS)

		OCG (1961 Income)		CENSUS (1969 Earnings)		PSID (1971 Earnings)	
		White (N=10,395)	Non-White (N=1110)	White (N=23,615)	Non-White (N=2082)	White (N=1260)	Non-White (N=514)
Earnings							
Years of Education	B	481.92 **	248.84	653.12 **	400.14	645.70 *	353.88
	s.e.	(17.98)	(21.51)	(18.94)	(30.19)	(74.51)	(89.26)
BA	B	1879.43	1751.49	2968.23 **	1609.33	2787.71 **	7920.40
	s.e.	(166.35)	(356.00)	(162.43)	(372.96)	(552.62)	(1066.33)
Experience	B	57.26 *	33.79	461.56 **	113.79	35.14	[11.25]
	s.e.	(4.15)	(6.85)	(15.01)	(30.34)	(14.59)	(24.37)
Experience ^{2}	B	-4.61 **	-1.32	-7.61 **	-1.47	-7.45	-6.57
	s.e.	(.30)	(.49)	(.29)	(.57)	(1.07)	(1.68)
Constant		6893.59	4278.08	9105.08	6707.21	9662.80	8251.21
R^2		.181	.191	.183	.163	.233	.285
S.D. of Resid.		4400.38	2277.57	6441.72	3836.63	5495.50	5353.71
Percentage of gap eliminated if equalization occurs at level of:							
Non-White means		40.7		45.2		56.1	
White means		23.3		26.5		53.7	
Earnings $^{1/3}$							
Years of Education	B	.4717	.4277	.4758	.4341	.5231 *	.2742
	s.e.	(.0152)	(.0329)	(.0111)	(.0281)	(.0524)	(.0592)
BA	B	.7234	[1.0499]	1.1259 *	[.3644]	1.4057 **	4.1430
	s.e.	(.1403)	(.5450)	(.0952)	(.3474)	(.3885)	(.7077)
Experience	B	.0366	.0485	.2976 **	.0962	[-.0008]	[-.0271]
	s.e.	(.0035)	(.0105)	(.0088)	(.0283)	(.0103)	(.0162)
Experience ^{2}	B	-.0038 *	-.0017	-.0052 **	-.0012	-.0073	-.0052
	s.e.	(.0003)	(.0008)	(.0002)	(.0005)	(.0008)	(.0011)
Constant		18.1528	15.3915	19.9732	17.9990	20.4500	18.7966
R^2		.185	.185	.209	.169	.276	.301
S.D. of Resid.		3.7114	3.4870	3.7753	3.5738	3.8636	3.5530
Percentage of gap eliminated if equalization occurs at level of:							
Non-White		32.4		35.7		47.9	
White means		30.3		29.7		39.3	

Table 8.8 continued

		OCG (1962 Income)		CENSUS (1969 Earnings)		PSID (1971 Earnings	
		White (N=10,395)	Non-White (N=1110)	White (N=23,615)	Non-White (N=2082)	White (N=1260)	Non-White (N=514)
Ln Earnings							
Years of Education	B	.0852	.1020	.0752	.0815	.0750	.0429
	s.e.	(.0029)	(.0081)	(.0019)	(.0059)	(.0082)	(.0151)
BA	B	[.0275]	[.0708]	.0901	[-.0343]	.1381 *	.6861
	s.e.	(.0268)	(.1342)	(.0160)	(.0729)	(.0612)	(.1804)
Experience	B	.0048	.0101	.0443 **	.0155	[-.0029]	-.0103
	s.e.	(.0007)	(.0026)	(.0015)	(.0059)	(.0016)	(.0041)
Experience^2	B	-.0006	-.0004	-.0008 **	[-.0002]	-.0012	-.0011
	s.e.	(.0000)	(.0002)	(.0000)	(.0001)	(.0001)	(.0003)
Constant		8.6074	8.0941	8.9143	8.5829	8.9829	8.6730
R^2		.150	.164	.171	.128	.248	.188
S.D. of Resid.		.7085	.8586	.6329	.7500	.6084	.9057
Percentage of gap eliminated if equalization occurs at level of:							
Black means		28.7		31.7		37.8	
White means		34.6		31.0		36.6	

* The ratio of the difference between whites and non-whites to the standard error of the difference is greater than 2 but less than 3

** The ratio of the difference between whites and non-whites to the standard error of the difference is equal to or greater than 3.

through 1971. In the equation predicting non-white Ln Earnings for 1969, the coefficient of BA is 0.392 ± 0.112 compared to -0.0343 ± 0.0729 in the Census. This suggests that the difference between the Census and PSID results for non-whites is at least partly due to measurement or sampling differences (e.g. the treatment of **respondents with Spanish surnames** as non-white and the exclusion of men who were not heads of their households in PSID). **The** equations computed for each year of the PSID also allow us to investigate time trends without worrying about the comparability of samples and questionnaires. The PSID sample may not be perfectly representative of the U.S. population, but **it seems reasonable** to assume that the biases are roughly constant from 1967 to 1971. Observed trends should therefore reflect real population trends.[11]

The white equations show no significant changes from 1967 to 1971. The coefficient of Years of Education in the non-white equations shows a steady decrease from 0.084 in 1967 to 0.040 in 1971. This is consistent with the time trend in Table 8.8. The coefficient of BA in the non-white equations rises from 0.208 to 0.744. The consistency of this trend over the five-year interval supports the view that the returns to education among non-whites really changed between 1967 and 1972.

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[11] We discussed overall discrepancies between the PSID 1971 earnings distribution and the CPS 1971 personal income distribution in the previous section. The differences between PSID and CPS are even greater when one compares PSID non-whites to CPS blacks. This may be partly caused by the coding of Spanish-Americans as non-whites in the PSID. Perhaps the greatest difference between the PSID and the CPS is that the standard deviation of Earnings for the PSID non-whites is the same **as for whites.** We would expect it to be much lower, given the usual monotonic relationship between the means and standard deviations of earnings distributions. This indicates that there are more non-whites with moderate to high earnings in the PSID sample than in the other samples.

Since the PSID longitudinal sample is quite small, we also looked at cross-sectional CPS data for the years 1967 through 1974. The comparison poses three problems. First, CPS does not publish data on earnings by educational level. Its data are for total personal income. Second, CPS does not publish income data by race for men 25-64, only for men 25 and over. Neither of these discrepancies would be serious in isolation, since most earners are 25-64 and most income of 25-64 year olds comes from earnings. But the two discrepancies together lead to the inclusion of large numbers of men 65 and over without earnings. A third source of difficulty is that since 1967 CPS has published income data for blacks rather than for all non-whites. Table 8.9 illustrates the effects of these changes in coverage and definition. Since the form for the table is novel, some explanation may be helpful.

The first column shows the ratio of non-white to white earnings for men 25-64 who were in the experienced civilian labor force in 1970. The data come from the 1/20 Census sample, so the sampling errors are negligible. If percentage returns to education are roughly similar for non-whites and whites, the ratio of nonwhite to white earnings will **be constant for all levels of education.** If returns are higher for nonwhites, the ratio of non-white to white earnings will / **increase at higher levels of education.** If returns are lower for non-whites, the ratio **will decrease.** **Reading** down column 1 we see that the ratio shows no consistent tendency either to rise or to fall. This suggests that returns to education are roughly similar for non-whites and whites. Returns to undergraduate education seem to be lower for non-whites, while returns to graduate education seem to be higher. The second column of the table shows analogous ratios from our 1/1000 sample. It is consistent with column 1, except that non-whites with graduate education do slightly worse relative to whites in our 1/1000 sample than in the larger 1/20 sample covered by column 1. Since our 1/1000 sample includes only 149 non-whites with graduate education, the sampling error of their mean earnings is substantial.

Column 3 shows that shifting from the non-white/white ratio to the black/white ratio leaves the overall picture unchanged, except that the 85 blacks with graduate education appear to earn 30 percent more than the other 64 non-whites with graduate education in our sample, so the black/white ratio is higher than the non-white/white ratio at the graduate level. Column 4 changes the measure of economic success from earnings to total income. This does not change the picture. Returns to education are as great for blacks as for whites. Column 5 is similar to column 4 except that it comes from the Census 1/20 sample. This means the sampling errors are much smaller. If we ignore the handful of blacks with graduate education, there is a consistent decline in the ratio of black to white income as education increases. This suggests that in terms of total income , returns to the first 16 years of education are lower for blacks than for whites. Our 1/1000 sample obscured this fact due to sampling error. Our black 8th graders have higher incomes than those in the 1/20 sample, and our black BA's have lower incomes than those in the 1/20 sample. Columns 6 and 7 extend the analysis to include men over 65. Now returns to secondary school

Table 8.9

RATIO OF MINORITY TO WHITE INCOME OR EARNINGS IN 1969: CENSUS 1/20 AND 1/1000 SAMPLES

	NON-WHITE/WHITE		BLACK/WHITE				
	EARNINGS RATIO 1/			INCOME RATIO 2/			
	MEN 25-64					MEN 25 AND OVER	
Education	1/20 Sample (1)	1/1000 Sample (2)	1/1000 Sample (3)	1/1000 Sample (4)	1/20 Sample (5)	1/1000 Sample (6)	1/20 Sample (7)
0 - 7	.698	.730	.716	.693	.738	.732	.745
8		.690	.681	.648	.698	.695	.762
9 - 11	.707	.671	.658	.644	.662	.660	.684
12	.722	.720	.705	.678	.670	.681	.677
13 - 15	.695	.695	.663	.630	.649	.643	.652
16	.650	.649	.675	.612	.579	.632	.573
17+	.752	.695	.756	.715	.695	.697	.689

1/ For civilian non-institutional non-students with non-zero earnings in 1/1000 sample. For experienced civilian labor force in 1/20 sample.

2/ For men with non-zero incomes.

Sources: column 1: 1970 Census of Population. Subject Reports, Earnings by Occupation and Education PC(2) - 8B, Table 1.
columns 2,3,4,6: Census 1/1000 County Group Sample.
columns 5 + 7: 1970 Census of Population, Detailed Characteristics U.S. Summary PC(1)-D1, Table 249.

and college are clearly lower for blacks 25 and over than for whites 25 and over even in our 1/1000 sample. The same holds for the larger 1/20 sample. Returns to graduate education are, however, still clearly higher for blacks. This may also be true for elementary education, but we cannot be sure, since blacks with 0 to 7 years of education typically have less education than whites in the same category. The ratio of black to white earnings among men with 0-7 years of education is therefore lower than the ratio for blacks and whites with exactly the same amount of education.

These data suggest that one should be cautious in generalizing from white/non-white earnings differentials to white/black income differentials. Nonetheless, if returns to higher education rose substantially for 25-64 year old non-whites between 1967 and 1972, as the PSID data suggest they did, this should also be apparent in CPS income data on whites and blacks 25 and over. Table 8.10 allows us to assess the magnitude of the change. It shows the ratio of non-white to white earnings at each educational level in each year. CPS sampled less than 300 black college graduates in any given year, so the black-white earnings ratio at that level has a standard error of around 0.04 in any given year. Nonetheless, Table 8.10 clearly suggests some improvement in the relative position of black college graduates during the early 1970's, when affirmative action was most vigorous. This is consistent with the PSID longitudinal results. Taken as a whole, however, Table 8.10 shows no clear trend in black-white ratios at any educational level. Thus if overall returns to education changed during these years, they must have changed at about the same rate for blacks as for whites.

Table 8.10

RATIO OF BLACK TO WHITE INCOMES BY EDUCATION AND YEAR:
MALES 25 AND OVER WITH INCOME

EDUCATION	CPS 1967	CPS 1968	CPS 1969	Census 1969	CPS 1970	CPS 1971	CPS 1973	CPS 1974
0 - 7	.799	.744	.778	.745	.789	.795	.745	.752
8	.753	.769	.766	.762	.754	.765	.734	.762
9 - 11	.690	.728	.698	.684	.722	.714	.799	.716
12	.684	.690	.678	.677	.695	.691	.731	.739
13 - 15	.673	.692	.667	.652	.684	.665	.677	.709
16 +	.650	.643	.631	.630	.694	.696	.694	.649

SOURCES: Current Population Reports, Series P-60.

U S. Bureau of the Census, 1970 Census of Population Detailed Characteristics, US Summary PC(1)-D1, Table 249.

It is not as easy as it should be to compare these results to earlier research on returns to education for whites and non-whites. Thurow (1969), Link (1975), and Weiss and Williamson (1975) all regressed Ln Earnings on Ln Education rather than Education. Such "double-log" equations explain less of the variance in Ln Earnings than our "semi-log" equations, and the results make less theoretical sense. (The double-log coefficients represent elasticities. The semi-log coefficients represent the percentage effect of an extra year of schooling.) The double log equations may be more like regressions of Earnings on Education than like regressions of Ln Earnings on Education. A second obstacle to comparisons is that **Weiss and Williamson** (1972, 1975) included men without earnings in their samples, assigning them $1.00. As McClelland shows in Chapter 16, including non-earners converts the dependent variable into a virtual dichotomy between labor force participants and non-participants.

The determinants of labor force participation are not the same as the determinants of earnings among those who participate, so results for samples that include non-participants will be very different from results for samples that exclude them. A third difficulty is that both Thurow and Link worked with published Census data in which both the education and earnings data are grouped. Weiss and Williamson (1975) argue that this has a significant effect on results.

Thurow's analysis of 1960 Census data suggested that the elasticity of earnings with respect to education in 1959 was somewhat higher for whites than for non-whites. Our semi-log OCG results for 1961 show no difference.

Link's analysis of published 1970 Census data suggested that the elasticity of earnings with respect to education rose between 1959 and 1969 for both whites and non-whites. This is consistent with Bartlett's results in Chapter 7. Link also concluded that the elasticity for whites exceeded that for blacks in 1969, and that the difference was almost as large as it had been in 1959.

Using 1970 Census data on individuals, Weiss and Williamson (1975) claimed that "the effect of education on earnings (in 1970) is roughly as strong for blacks as for whites" (1975: 244). They argued that their results differed from those of Thurow and Link largely because they used individual rather than grouped data. But their results also differ from Link's (and ours) by including non-earners. We can see no reason for revising our conclusions on the basis of these comparisons. There is no strong evidence that extra years of education raise earnings by a larger percentage among 25-64 year old whites than among 25-64 year old nonwhites. There is evidence that extra education raises income more among whites 25-64 than among blacks 25-64. We cannot pinpoint the reason for this difference using the 1/1000 sample, though the 1/100 sample would allow us to do so.

Experience and Earnings

The Ln Earnings equations in Table 8.8 show that returns to experience are different for whites and non-whites. With a quadratic experience term in the equation, we find the net return to an additional year of experience (holding education constant) by computing the partial derivative of the equation with respect to experience. The partial derivative of the Census Ln Earnings equation for whites is 0.0443 + (2)(-0.0008) (Experience), where Experience is the average number of years of experience one has in the interval. The implied return to the first year of job experience for whites is thus 0.0443 + (2)(-0.0008)(0.5) = 0.0451 or about 4.6 percent. The marginal value of an additional year of job experience for those who already have 25 years of experience is 0.0443 + (2) (-0.0008)(25.5) = .0035 or 0.35 percent. Non-whites receive much lower returns to initial job experience (1.55 percent) but their returns decline much more slowly (-0.04 percent per year instead of (2) (-0.0008).

-0.16 percent. Thus after 25 years of experience the marginal value of additional experience for non-whites is still 0.55 percent, which is greater than that of whites.

We cannot analyze the differential effects of experience in the OCG and PSID without adjusting for the orthogonalization of the Experience^2 variable. When this is done, the experience profiles of whites and non-whites are more similar than in the Census. Nonetheless, the same general pattern prevails: higher returns to initial experience for whites accompanied by greater declines in the return to additional years of experience.

As long as the value of additional experience is greater for whites, the gap between white and non-white earnings will increase at higher levels of experience. In the Census this implies that with each year of experience up to 24 years (when the marginal value is equal for whites and non-whites) the difference between Ln Earnings for whites and non-whites increases, albeit at a diminishing rate. The gap reaches a maximum at 24 years of experience and then begins to decrease. In the OCG, the estimated gap between whites and non-whites rises until men have 12 years of experience and then begins to fall. In the PSID, the estimated gap between whites and non-whites continues to increase up to 50 years of experience. The divergence of these results should underline the fact that the calculations all have large sampling errors. Even more serious, earnings profiles are not in fact parabolas, so derivatives based on equations that use Experience and Experience^2 will not yield precise estimates of the marginal value of additional experience. This problem is particularly acute at the extremes of the Experience distribution. Samples of 25-64 year olds include few men with either zero or fifty years of experience, so the derivatives at these points can be (and are) seriously misleading. Nonetheless, all analyses show that the ratio of white to non-white earnings is higher for middle aged men than for young men.

Effects of Equalizing Education and Experience

We now turn to the question of how much of the earnings gap between whites and non-whites is due to differing amounts of education and experience. In analyzing the contribution of educational differences to economic inequality between whites and non-whites, it is important to bear in mind that such inequality diminished between 1961 and 1971. Row 1 of Table 8.11 shows the ratio of non-white to white earnings in each of the three surveys, using each of our three measures of economic success. Row 2 shows the estimated ratio of non-white to white earnings for men with the same distribution of education and experience as the present non-white population. Row 3 shows the estimated ratio for men with the same distribution of education and experience as the present white population. This type of analysis is clearly sensitive to the form of the dependent variable. Rather than assume that the "correct" measure is either dollars, log dollars, or cube root dollars, we present results for all three measures of earnings.

From a social policy perspective, there are several ways of eliminating the racial gap in earnings. The first would be to attempt to equalize educational levels in the current generation of workers. Since one cannot take education away from already-educated whites, the problem would be to increase the education of non-white adults. Past experiences with adult education and job training programs suggest that this type of policy can be quite expensive and, while benefiting certain individuals, is unlikely to have a large effect on the

Table 8.11

RATIOS OF NON-WHITE TO WHITE EARNINGS/INCOME FOR MEN AGED 25-64 WITH COMPLETE DATA AND POSITIVE EARNINGS/INCOME

	OCG (1961)			Census (1969)			PSID (1971)		
	Income	Income$^{1/3}$	Ln Income	Earnings	Earnings$^{1/3}$	Ln Earnings	Earnings	Earnings$^{1/3}$	Ln Earnings
1) Total Sample[a/]	.504	.480	.459	.639	.634	.618	.689	.649	.613
2) Men with Non-white Distribution of Education and Experience	.706	.648	.614	.802	.765	.739	.863	.817	.759
3) Men with White Distribution of Education and Experience	.620	.638	.646	.734	.743	.736	.856	.786	.775
4) Men with Non-White Distribution of Background	.675	.632	.602	–	–	–	.809	.770	.721
5) Men with White Distribution of Background	.599	.579	.556	–	–	–	.877	.795	.765

a/ Ratio for Income$^{1/3}$ = (Mean Nonwhite Income$^{1/3}$/Mean White Income$^{1/3}$)^3

Ratio for Ln Income = Anti-log (Mean Nonwhite Ln Income - Mean White Ln Income)

We generated these ratios as follows. Let f_w and f_n denote the regression equations for whites and non-whites respectively. Let X_w and X_n denote the vector of means for whites and non-whites respectively. Let $f_w(X_w)$ denote the value obtained by substituting the white means in the white equation, and so forth. Then line 1 = $f_n(X_n)/f_w(X_w)$. Lines 2 and 4 = $f_n(X_n)/f_w(X_n)$. Lines 3 and 5 = $f_n(X_w)/f_w(N_w)$.

mean earnings of non-whites; at most, it could reduce the gap by only 25-35 percent (35-50 percent in the PSID data). The second alternative is to concentrate on encouraging young non-whites to obtain more education, and hopefully bring their mean level of education up to the white level. This policy has the advantage of greater feasibility, but is of little benefit to the current non-white adult population.

If the non-white educational distribution were eventually to approximate the white distribution, and if other causally subsequent sources of inequality were to remain unaltered, Table 8.11 implies that between 25 and 50 percent of the present earnings difference between whites and non-whites would disappear. This estimate must obviously be treated cautiously. First, a large increase in the supply of educated non-white workers might well lead to a decline in their relative wages. Then too, unless the entire effect of education on earnings is due to the fact that extra education raises productivity, increasing mean non-white education will not increase GNP enough to cover the expected increase in earnings. The only way to increase the earnings of highly educated non-whites to the expected level will then be to reduce the real wages of whites. (This could be done by pay cuts or through general inflation.) Finally, part of the apparent effect of schooling is really due to causally prior traits like parental status and ability. Unless the means for these traits increased along with education, the benefits of education would be less than our equations imply. The estimates in Table 8.11 therefore constitute an upper bound on the likely effects of redistributing education. (Differences in experience explain less than one percent of the white/non-white earnings differential). Even if we ignore (as we have) the possible development of new or alternative forms of discrimination, changing non-white educational attainment is likely to have less effect than our estimates indicate.

An alternative policy is to insure that whites and non-whites with equal levels of education and experience receive equal pay. We can estimate the effect of this change by substituting the non-white means on education and experience into the white equations. The result would be a 55-70 percent reduction in the gap between white and non-white earnings.[13/] Public policies that ensure that non-whites earn as much as whites with similar education have several advantages over policies that increase non-white educational attainment, though both are clearly desirable. First, the benefits are likely to be felt sooner, since changing mean education is a gradual process and would be of little value to older workers. Second, the estimated effect

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[13/] Using the notation at the bottom of Table 8.11, the average income of non-whites assuming they received equal pay for equal credentials is $f_w(X_n)$. This is the same as the hypothetical white average income assuming that white means on the independent variablas equalled non-white means, i.e., $f_w(X_n)$. However, in the first case we are asking how much this change moves non-whites towards whites, i.e., $[f_w(X_n) - f_n(X_n)]/[f_w(X_w) - f_n(X_n)] =$ (55-70 percent), and we therefore compare the hypothetical non-white average to the observed non-white average. In the latter situation we ask how different (as a percentage of the total white/non-white gap) the hypothetical white average income is from the observed white average income $[f_w(X_w) - f_w(X_n)]/[f_w(X_w)-f_n(X_n)] =$ (30-45 percent). These two cases split the total white/non-white gap into two pieces and it follows that the sum of the two pieces will always equal 100 percent of the gap:

$$\frac{f_w(X_w)-f_w(X_n)}{f_w(X_w)-f_n(X_n)} + \frac{f_w(X_n) - f_n(X_n)}{f_w(X_w) - f_n(X_n)} = \frac{f_w(X_w) - f_n(X_n)}{f_w(X_w) - f_n(X_n)} .$$

of equalizing white/non-white earnings within educational levels is generally greater than the estimated effect of equalizing the distribution of education. Furthermore, our analysis probably overestimates the actual effect of equalizing the distribution of education. This implies that we probably underestimates the effects of equalizing earnings for equally qualified whites and non-whites. Finally, if non-white returns to education were increased to white levels, the advantages of continuing one's education would be greater and mean non-white education would probably increase, creating further increases in non-white earnings.

Social Background and Earnings

We now turn to an examination of the relationship between social background and earnings. Table 8.12 presents regressions of the earnings variables on background characteristics in the OCG and PSID samples. The white and non-white coefficients of Father White Collar, Father Absent, and Non-South Upbringing do not differ significantly.

The coefficients of the other variables present a confusing picture. This seems to be partly because **white/non-white differences in the effect of a variable are reflected in the coefficient of the linear term in one sample and in the coefficient of its orthogonal squared term in the other sample.** When predicting Cube Root and Ln Earnings in OCG, for example, the coefficient of Father's Occupation is significantly higher for whites than for non-whites. The difference between the white and nonwhite coefficients of Father's Occupation^2 is significant in the PSID. Similarly, in the OCG Income equation Siblings has a significantly larger coefficient for whites than non-whites. In the PSID the difference is for Siblings^2. Non-Farm Upbringing behaves more erratically than any of the other variables. The OCG and PSID samples agree that the difference between the white and non-white coefficients is significant in the Cube Root equation, but the difference is in the

Table 8.12

REGRESSIONS OF EARNINGS$^{1/3}$ AND EARNINGS ON BACKGROUND CHARACTERISTICS FOR MALES AGED 25-64 WITH POSITIVE EARNINGS, BY RACE (1967 DOLLARS)

		OCG Income		PSID Earnings	
		White (N=10, 395)	Non-White (N=1110)	White (N=1260)	Non-White (N=514)
Father's Education	B	80.31	79.66	199.67	[81.39]
	s.e.	(13.45)	(23.40)	(62.60)	(105.74)
Father's Occupation	B	36.16 **	[-8.98]	[-42.58]	[-36.71]
	s.e.	(3.68)	(9.74)	(21.67)	(35.16)
Father White Collar	B	585.42	[238.57]	2590.72	4939.72
	s.e.	(170.66)	(435.37)	(835.60)	(1732.78)
Father Absent	B	-487.30	-486.25	[-1104.17]	[-2327.66]
	s.e.	(131.67)	(173.95)	(2219.03)	(1626.07)
Non-South Upbringing	B	898.37	842.52	1047.99	1597.18
	s.e.	(106.59)	(162.38)	(400.80)	(639.50)
Non-Farm Upbringing	B	810.20	963.12	1776.02	[535.73]
	s.e.	(119.01)	(181.68)	(438.05)	(590.15)
Siblings	B	-120.45 *	-46.99	-212.09	-460.53
	s.e.	(15.80)	(23.03)	(69.04)	(105.59)
Father's Education^2	B	9.00	[1.26]	[3.88]	-33.16
	s.e.	(2.21)	(3.72)	(12.20)	(16.55)
Father's Occupation^2	B	[.06] *	-.45	[-.63]	[-.30]
	s.e.	(.10)	(.22)	(.56)	(.87)
Siblings^2	B	8.20	[1.10]	[38.28] *	134.86
	s.e.	(3.82)	(4.47)	(24.27)	(37.98)
Constant		6914.57	4145.50	9720.57	8441.11
R^2		.111	.100	.079	.165
S.D. of Resid.		4586.99	2408.73	6038.60	5820.06

Percentage reduction in gap if equalization occurred at level of:

	OCG	PSID
Non-White means	34.5	38.2
White means	19.1	60.6

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* The ratio of the difference to the standard error of the difference is greater than 2 but less than 3.

** The ratio of the difference to the standard error of the difference is equal to or greater than 3.

Table 8.12 continued

		OCG Income$^{1/3}$		PSID Earnings$^{1/3}$	
		White (N=10,395)	Non-White (N=1110)	White (N=1260)	Non-White (N=514)
Father's Education	B	.0802 *	.1647	.1554	[.0908]
	s.e.	(.0113)	(.0358)	(.0450)	(.0706)
Father's Occupation	B	.0292 **	[-.0287]	[-.0187]	[-.0317]
	s.e.	(.0031)	(.0149)	(.0156)	(.0235)
Father White Collar	B	.3211	[.7415]	1.4196	3.0437
	s.e.	(.1437)	(.6656)	(.6005)	(1.1576)
Father Absent	B	-.6033	[-.4965]	[-.3410]	[-1.2262]
	s.e.	(.1109)	(.2659)	(1.5946)	(1.0863)
Non-South Upbringing	B	.9410	.8410	.7632	1.3299
	s.e.	(.0898)	(.2482)	(.2880)	(.4272)
Non-Farm Upbringing	B	.9881 *	1.5959	1.4121 *	[.5387]
	s.e.	(.1002)	(.2778)	(.3148)	(.3942)
Siblings	B	-.1082	[-.0622]	-.1737	-.2809
	s.e.	(.0133)	(.0352)	(.0496)	(.0705)
Father's Education^2	B	[.0030]	[.0041]	[-.0030]	-.0273
	s.e.	(.0019)	(.0057)	(.0088)	(.0111)
Father's Occupation^2	B	[-.0001]	-.0008	[-.0005] *	[.0010]
	s.e.	(.0001)	(.0003)	(.0004)	(.0006)
Siblings^2	B	[.0056]	[.0005]	[.0208]	.0743
	s.e.	(.0032)	(.0068)	(.0174)	(.0254)
Constant		18.1651	14.9874	20.4888	18.8528
R^2		.118	.096	.092	.173
S.D. of Resid.		3.8622	3.6826	4.3394	3.8881

Percentage reduction in gap if equalization occurred at level of:

Non-White means	29.2	34.5
White means	19.0	41.6

Table 8.12 continued

		OCG Ln Income		PSID Ln Earnings	
		White (N=15,395)	Non-white N= 1140)	White (N=1260)	Non-White N=514
Father's Education	B	.0145 **	.0421	.0220	.0223
	s.e.	(.0021)	(.0088)	(.0070)	(.0174)
Father's Occupation	B	.0047 **	-.0082	[-.0025]	[-.0076]
	s.e.	(.0006)	(.0036)	(.0024)	(.0058)
Father White Collar	B	[.0401]	[.1924]	[.1869]	.6408
	s.e.	(.0272)	(.1630)	(.0935)	(.2850)
Father Absent	B	-.1194	[-.0788]	[-.0096]	[-.2212]
	s.e.	(.0210)	(.0651)	(.2482)	(.2674)
Non-South Upbringing	B	.1724	.1457	.1106	.2152
	s.e.	(.0170)	(.0608)	(.0448)	(.1052)
Non-Farm Upbringing	B	.2031 *	3.706	.2057	[.0576]
	s.e.	(.0189)	(.0680)	(.0490)	(.0970)
Siblings	B	-.0182	[-.0136]	-.0247	-.0511
	s.e.	(.0025)	(.0086)	(.0077)	(.0174)
Father's Education^2	B	[-.0000]	[.0011]	[-.0007]	-.0055
	s.e.	(.0004)	(.0014)	(.0014)	(.0027)
Father's Occupation^2	B	[-.0000]	-.0002	[-.0001] *	[.0003]
	s.e.	(.0000)	(.0001)	(.0001)	(.0001)
Siblings^2	B	[.0008]	[.0002]	[.0023]	[.0092]
	s.e.	(.0006)	(.0017)	(.0027)	(.0062)
Constant		8.6090	7.9769	8.9880	8.6856
R^2		.099	.083	.077	.103
S.D. of Resid.		.7297	.9018	.6755	.9571

Percentage reduction in gap if equalization occurred at level of:

Non-White means	26.5	28.0
White means	17.9	39.5

opposite direction in the two samples.

The three OCG equations are fairly consistent in their predictions of how much of the income gap could have been eliminated in 1961 by equalizing white and non-white means on the independent variables. For whites and non-whites at the non-white mean on the background variables the gap is about 30 percent less than in the population as a whole. If we compare whites to non-whites at the white mean, the gap is only about 10 percent less than the gap in the population as a whole. Once again, we may draw the conclusion that the earnings gap is wider at higher 'levels' of background. However, the PSID equations tell a different story. Like the OCG equations, they indicate that 28 to 38 percent of the gap would be eliminated if whites had the non-white means. Unlike OCG, however, 40 to 60 percent of the gap disappears for non-whites at the white mean in PSID. This implies a smaller gap at higher levels of background.

Table 8.11 shows the overall difference between whites and non-whites in each survey, along with the estimated difference among men with the same distribution of backgrounds as the white population and among men with the same distribution of backgrounds as the non-white population. As noted earlier, the overall gap is smaller in 1971 than in 1961. Background also seems to account for as much or more of the gap in 1971 than 1961. The unexplained gap is thus smaller.

Table 8.13 presents regressions for the OCG and PSID samples of the earnings variables on background, education, experience and occupation. The results dealing with background characteristics are essentially the same as those presented in Table 8.12. Most importantly, we see that for the OCG, even after controlling education, experience, and occupation, having a father in a high-status occupation continues to be a significant advantage to whites but no advantage at all to non-whites. Similarly the coefficient of Father's Occupation^2 continues to be higher for non-whites than whites in the PSID sample. The only striking difference between the two tables is that once education, experience, and occupation are controlled, the PSID exhibits significant differences between whites and non-whites on Siblings and Father's Education^2. The coefficients of these variables indicate that having had a large number of siblings is a much greater handicap to non-whites than to whites, and that the marginal value of Father's Education declines steeply for non-whites at higher levels of education. This last result implies an inability on the part of highly-educated non-whites to pass on their advantages to their children as effectively as whites do.

In neither sample does Father White Collar or Father Absent have any independent influence on earnings for either group once we control Education, Experience and Occupation. Non-Farm Upbringing and Non-South Upbringing play an important and similar role for both groups in the OCG but not the PSID equations.

For both samples, Years of Education shows a significant difference between whites and non-whites in predicting Cube Root Earnings and Ln Earnings, but not plain dollar earnings. The white coefficients do not differ significantly in the two samples, but the non-white coefficients go from being significantly greater than the white coefficients in the OCG sample to being significantly smaller in the PSID. The other two education variables show no consistent differences

Table 8.13

REGRESSIONS OF EARNINGS, CUBE ROOT OF EARNINGS AND LN EARNINGS ON BACKGROUND CHARACTERISTICS, EDUCATION, EXPERIENCE AND OCCUPATION FOR MALES AGED 25-64 WITH POSITIVE EARNINGS, BY RACE

(1967 dollars)

		OCG Income		PSID Earnings	
		White (N=10,395)	Non-White (N=1110)	White (N=1260)	Non-White (N=514)
Father's Education	B	[12.16]	[24.92]	[64.46]	[-148.24]
	s.e.	(12.59)	(22.44)	(58.28)	(98.91)
Father's Occupation	B	17.89 **	[-14.61]	[-35.40]	[-27.39]
	s.e.	(3.36)	(8.95)	(19.45)	(31.33)
Father White Collar	B	[47.43]	[-187.51]	[1205.64]	[1503.68]
	s.e.	(155.47)	(402.28)	(754.72)	(1566.05)
Father Absent	B	[-100.02]	[-121.97]	[-402.79]	[-278.87]
	s.e.	(120.17)	(163.79)	(1987.80)	(1451.30)
Non-South Upbringing	B	674.72	457.08	[594.74]	[1016.28]
	s.e.	(97.56)	(153.85)	(364.69)	(583.35)
Non-Farm Upbringing	B	449.40	615.82	1237.75	[704.09]
	s.e.	(108.88)	(172.03)	(395.75)	(528.83)
Siblings	B	-32.33	[-22.01]	[-6.55] **	-426.20
	s.e.	(14.68)	(21.26)	(63.67)	(92.68)
Father's Education^2	B	6.40	[1.36]	[5.45] **	-62.13
	s.e.	(2.03)	(3.44)	(11.04)	(15.00)
Father's Occupation^2	B	[.11]	[-.06]	[-.15]	[.36]
	s.e.	(.09)	(.21)	(.50)	(.80)
Siblings^2	B	[3.15]	[1.75]	[22.77] *	119.91
	s.e.	(3.47)	(4.11)	(21.98)	(33.36)
Years of Education	B	177.70	187.65	370.55	[94.57]
	s.e.	(21.88)	(24.58)	(101.38)	(107.41)
Years of Ed. Past High School	B	[-14.31]	[-214.45]	[242.19]	[436.93]
	s.e.	(67.05)	(139.43)	(237.27)	(437.01)
BA	B	880.84	1668.14	[1530.84]	5045.05
	s.e.	(285.43)	(649.16)	(866.73)	(2146.45)
Experience	B	44.32	32.87	35.98	[28.76]
	s.e.	(3.99)	(6.83)	(14.70)	(24.46)
Experience^2	B	-5.02 **	-1.58	-7.47	-7.97
	s.e.	(.29)	(.49)	(1.07)	(1.67)
Occupation	B	63.41 **	30.42	50.23	63.57
	s.e.	(2.18)	(4.85)	(9.58)	(17.54)
Constant		6808.04	4681.08	9623.40	9452.33
R^2		.270	.247	.265	.379
S.D. of Resid.		4156.94	2209.20	5405.21	5048.69

Percentage reduction in gap if equalization occurred at level of:

	OCG Income	PSID Earnings
Non-White means	66.2	68.3
White means	36.1	97.5

Table 8.13 continued

		OCG Income$^{1/3}$		PSID Earnings$^{1/3}$	
		White (N=10,395)	Non-White (N=1110)	White (N=1260)	Non-White (N=514)
Father's Education	B	[.0115]	.0772	[.0416]	[-.0540]
	s.e.	(.0106)	(.0346)	(.0407)	(.0646)
Father's Occupation	B	.0135 **	-.0364	[-.0160]	[-.0293]
	s.e.	(.0028)	(.0138)	(.0136)	(.0205)
Father White Collar	B	[-.0868]	[.2784]	[.5272]	[1.0387]
	s.e.	(.2304)	(.6201)	(.5273)	(1.0231)
Father Absent	B	-.2144	[.0857]	[.0985]	[.6302]
	s.e.	(.1008)	(.2525)	(1.3887)	(.9481)
Non-South Upbringing	B	.7362	[.2926]	[.3782]	[.5314]
	s.e.	(.0818)	(.2372)	(.2548)	(.3811)
Non-Farm Upbringing	B	.6108	.9481	.8746	[.6102]
	s.e.	(.0913)	(.2652)	(.2765)	(.3455)
Siblings	B	[-.0219]	[-.0224]	[-.0030] **	-.2564
	s.e.	(.0123)	(.0328)	(.0445)	(.0606)
Father's Education^2	B	[.0029]	[.0054]	[.0029] **	-.0433
	s.e.	(.0017)	(.0053)	(.0077)	(.0098)
Father's Occupation^2	B	[.0000]	[-.0003]	[-.0001] **	.0018
	s.e.	(.0001)	(.0003)	(.0004)	(.0005)
Siblings^2	B	[.0012]	[.0020]	[.0114]	.0635
	s.e.	(.0029)	(.0063)	(.0154)	(.0218)
Years of Education	B	.2472 **	.3781	.3670 *	[.1199]
	s.e.	(.0184)	(.0379)	(.0708)	(.0702)
Years of Ed. Past High School	B	-.3098	-.7367	[-.0955]	[-.1393]
	s.e.	(.0562)	(.2149)	(.1658)	(.2855)
BA	B	1.0175	2.8384	[1.1719]	3.3698
	s.e.	(.2394)	(1.0006)	(.6055)	(1.4022)
Experience	B	.0260	.0480	[.0017]	[-.0142]
	s.e.	(.0034)	(.0105)	(.0103)	(.0160)
Experience^2	B	-.0040 *	-.0018	-.0071	-.0059
	s.e.	(.0002)	(.0008)	(.0008)	(.0011)
Occupation	B	.0559 *	.0367	.0456	.0648
	s.e.	(.0018)	(.0075)	(.0067)	(.0115)
Constant		18.0662	15.6915	20.4102	19.4725
R^2		.281	.231	.315	.412
S.D. of Resid.		3.4871	3.4052	3.7762	3.2982

Percentage reduction in gap if equalization occurred at level of:	OCG	PSID
Non-White means	55.0	61.4
White means	38.6	66.6

Table 513 continued

		OCG Ln Income		PSID Ln Earnings	
		White (N=10,395)	Non-White (N=1110)	White (N=1260)	Non-White (N=514)
Father's Education	B	[.0020] *	.0210	[.0048]	[.0021]
	s.e.	(.0020)	(.0085)	(.0064)	(.0171)
Father's Occupation	B	.0021 **	-.0100	[-.0024]	[-.0072]
	s.e.	(.0006)	(.0034)	(.0022)	(.0054)
Father White Collar	B	[-.0220]	[.1065]	[.0761]	[.2887]
	s.e.	(.0252)	(.1532)	(.0833)	(.2703)
Father Absent	B	-.0486	[.0591]	[.0428]	[.1171]
	s.e.	(.0195)	(.0624)	(.2195)	(.2505)
Non-South Upbringing	B	.1354	[.0216]	[.0536]	[.0227]
	s.e.	(.0158)	(.0586)	(.0403)	(.1007)
Non-Farm Upbringing	B	.1340	.2080	.1176	[.0706]
	s.e.	(.0177)	(.0655)	(.0437)	(.0913)
Siblings	B	[-.0027]	[-.0042]	[.0008] *	-.0466
	s.e.	(.0024)	(.0081)	(.0070)	(.0160)
Father's Education^2	B	[.0002]	[.0015]	[.0006] **	-.0081
	s.e.	(.0003)	(.0013)	(.0012)	(.0026)
Father's Occupation^2	B	[.0000]	[-.0001]	[-.0000] *	.0004
	s.e.	(.0000)	(.0001)	(.0001)	(.0001)
Siblings^2	B	[.0001]	[.0006]	[.0011]	[.0077]
	s.e.	(.0006)	(.0016)	(.0024)	(.0058)
Years of Education	B	.0523 **	.0959	.0581 *	[.0093]
	s.e.	(.0036)	(.0094)	(.0112)	(.0185)
Years of Ed. Past High School	B	-.0846 *	-.2077	[-.0350]	[-.0154]
	s.e.	(.0109)	(.0531)	(.0262)	(.0754)
BA	B	.2038	.6615	[.1582]	[.3890]
	s.e.	(.0463)	(.2471)	(.0957)	(.3705)
Experience	B	.0032 *	.0102	[-.0026]	-.0085
	s.e.	(.0006)	(.0026)	(.0016)	(.0042)
Experience^2	B	-.0006	[-.0004]	-.0012	-.0013
	s.e.	(.0000)	(.0002)	(.0001)	(.0003)
Occupation	B	.0093	.0077	.0070 *	.0144
	s.e.	(.0004)	(.0018)	(.0011)	(.0030)
Constant		8.5919	8.1282	8.9767	8.7807
R^2		.232	.207	.283	.266
S.D. of Resid.		.6742	.8411	.5968	.8714

Percentage reduction in gap if equalization occurred at the level of:

	OCG	PSID
Non-White means	49.3	49.7
White means	39.4	61.1

* The ratio of the difference to the standard error of the difference is greater than 2 but less than 3.

between races. The experience variables show some differences, but they have very little impact on the earnings differential.

Note that the education coefficient in these equations captures only direct effects with Occupation controlled, not *total* effects. Note, too, that the group with the larger Education coefficient has the smaller Occupation coefficient. It appears from this and an examination of the Education coefficients without Occupation controlled that Education affects Earnings more through its effect on Occupation among PSID non-whites than among PSID whites. The reverse is true for the OCG sample.[14]

We now examine the overall effect on earnings of white/nonwhite differences in the independent variables. If OCG non-whites acquired white means, the gap would be reduced by about 38 percent in all three equations. If OCG whites acquired non-white means, the gap would be reduced by 50 percent in Ln Income, by 55 percent in Income $^{1/3}$, and by 66 percent in dollar Income. These figures indicate that in the OCG sample the income gap increases as the level of background, education and occupation increases.

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[14] The NLS Earnings equation has significant White x Education and White x Father's Occupation interactions. The Ln Earnings equation has only a White x Education interaction. **These are generally consistent with the discussion of the OCG and PSID equations.**

If PSID whites acquired non-white means, the gap in Earnings, Earnings$^{1/3}$ and Ln Earnings would be reduced by approximately the same amount as income was in the OCG sample. However, if PSID non-whites acquired white means, the gap in Ln Earnings would be reduced by 51 percent, the gap in Earnings$^{1/3}$ by 67 percent, and the gap in Earnings by a full 98 percent. Thus we see that for Earnings$^{1/3}$ and Ln Earnings, the gap decreases as the level of background, education and occupation increases, reversing the pattern in the OCG sample.

In describing the situation where non-whites acquire white means, the PSID assigns almost full responsibility for the gap in Earnings to differential means on the independent variables. In other words, if we compare the average white to a non-white from a similar background who had a comparable education and occupation, the expected difference in their Earnings would be 2 percent of the average white/non-white difference, or about $77. This is a much smaller difference than in any of our other samples. Given the skewness of the income distribution, we interpret this result as implying that those non-whites in the PSID sample who have a good background, good education, and a high status occupation earn at least as much and probably more than comparable whites. This was observed in the discussion of education and earnings. The earnings transformations (Earnings$^{1/3}$, Ln Earnings) tend to diminish the effect of these individuals on the regression as a whole. The equations using these transformations indicate that between 40 and 70 percent of the white/non-white difference in earnings is associated with differences in background, education and occupation. The remaining 30 to 60 percent of the gap is due to differences in earnings between whites and non-whites with equivalent background, education and occupation.

During the ten years between the OCG and PSID surveys, the average earnings of both whites and non-whites rose considerably, even after controlling for inflation. It seems useful to ask how these increases related to changes in average background, education, and experience of whites and non-whites.

Let us begin with whites. Average white earnings in the PSID were $2813 greater than average white income in the OCG. If we insert the 1961 white means on background, education, experience, and occupation in the 1971 equation, we find that changes in these characteristics between 1961 and 1971 account for $333 of the $2813 increase. This implies that 88 percent of the white increase was due to factors other than changes in background, education, experience and occupation. If we reverse the process and substitute the 1971 means in the 1961 equation, we find that changes in background, education, experience, and occupation imply a $30 decline in earnings between 1961 and 1971. The estimated contribution of changes in average background derived from the equations for Earnings$^{1/3}$ and Ln Earnings also falls within the above 0-12 percent range. It seems reasonable to conclude that the changes in background, education, experience, and occupation (as measured by our variables) during the 1960's had very little effect on earnings. If change in average earnings (after controlling for inflation) is associated with change in productivity, then productivity evidently rose independently of those traits we have examined in this chapter.[15]

The picture is slightly different for non-whites. Using the OCG non-white equation, changes in the average non-white's background explain 7 to 11 percent of the increase in non-white incomes between 1961 and 1971. This is just what we found for whites. However, when we use the PSID non-white equations, changes in background, education, experience, and occupation account for between 57 and 100 percent of the observed increase in non-white earnings between 1961 and 1971 (the exact value depends on the transformation used). This means that the typical 1961 non-white fared very poorly in 1971. It was the non-whites who had more education and had entered higher status occupations who accounted for most of the improvement in mean non-white earnings between 1961 and 1971.

Conclusion

It is natural to ask how the results of these regression analyses relate to the concept of social discrimination. If we assume that the equations include all variables that affect productivity, any part of the gap in earnings not directly caused by differences in means on the independent variables must result from 'direct' labor market discrimination. In fact, of course, the equations are unlikely to include all the relevant determinants of productivity. The meaning of the unexplained 'gap' is therefore open to debate.

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[15] While the increase in productivity may be associated with other individual characteristics, we suspect that much of it is due to improved technology.

Even differences between white and non-white [illegible]s on background and education are arguably the result of past discrimination. Because racial discrimination has been operating for so long, non-whites are concentrated in the lower classes and suffer from class handicaps as well as racial ones: larger families, fewer fathers present, and so forth. Non-whites have not received the same encouragement to become educated, they have not had the same access to higher education, and even when they have managed to get a good education, they have certainly not received equal consideration from employers. In the historical sense, one might attribute all the differences in means (except perhaps for differences due to experience) to racial discrimination. While racial discrimination has far-reaching effects (discriminate against a man, and not only do you harm him, but you harm his children's chances of advancing), certain policies which **seek** to eliminate the gap in earnings have a similarly double-edged, but positive,effect. If you improve educational prospects for non-whites, you indirectly help their children, as the mean of Father's Education will rise in the next generation. Similarly, eliminating job discrimination would improve the non-white mean on Father's Occupation and Father White Collar in the next generation. While such changes do not reduce the earnings gap quickly, they do lead toward the desired goal. These long-term policies should be accompanied by the more direct policy of reducing pay differentials between equally qualified whites and non-whites.

Appendix 8A

A COMPARISON OF BLACKS AND OTHER NON-WHITES IN THE 1970 CENSUS

In the Census sample blacks earned an average of $3332 less than whites and $2133 less than other non-whites. These other non-whites are more similar to whites than to blacks. Table A1 presents regression equations showing how this relationship changes when one controls Education and Experience. Controlling these variables (but excluding interactions) the first Earnings equation shows that when blacks have as much education as other non-whites, the earnings differential between them drops from $2133 to a not quite significant $697, while the analogous differential between blacks and whites decreases to $2335. Comparing those with equal education and experience, other non-whites are much more similar to blacks than to whites.

The second equation adds a variety of possible multiplicative interactions between education and experience. (These interaction terms were not made orthogonal to the additive terms.) The differences between blacks and other non-whites now become significant. However, the non-whites are still more similar to blacks than whites (when we control education and experience), reaffirming the above conclusion. The regressions of Earnings$^{1/3}$ and Ln Earnings again yield similar results.

Thus if one tries to explain the differential between blacks and whites with equal amounts of education by invoking theories of genetic or cultural differences, one must be prepared to argue that other non-whites suffer from many of the same genetic or cultural disadvantages as blacks. Alternatively, if one argues that the earnings differential is due to labor market discrimination, one should be prepared to argue that discrimination affects other non-whites almost as severely as blacks.

Table A1

REGRESSIONS OF EARNINGS, EARNINGS$^{1/3}$ AND LN EARNINGS ON EDUCATION, EXPERIENCE AND RACE FOR MALES AGED 25-64 WITH POSITIVE EARNINGS AND REPORTING EDUCATION: 1970 CENSUS 1/1000 SAMPLE

		Earnings			Earnings$^{1/3}$			Ln Earnings		
Years of Education	B	531.42	455.44	342.79	.4614	.5833	.5644	.0772	.1118	.1169
	s.e.	(21.05)	(137.65)	(148.16)	(.0123)	(.0808)	(.0870	(.0020)	(.0134)	(.0145)
Years of Higher Education	B	670.15	[152.15]	[-83.90]	.1113	[-.1770]	[-.2095]	[-.0095]	-.0603	[-.0604]
	s.e.	(58.35)	(224.45)	(321.35)	(.0342)	(.1318)	(.1888)	(.0057)	(.0219)	(.0314)
BA	B	687.87	-1937.72	[-2581.62]	.7420	[-.9465]	[-1.6844]	.1197	[-.1004]	[-.2255]
	s.e.	(271.03)	(876.34)	(1472.31)	(.1588)	(.5146)	(.8649)	(.0263)	(.0855)	(.1437)
Experience	B	473.68	[184.43]	[-14.14]	.2927	.2491	.1340	.0417	.0487	.0327
	s.e.	(14.84)	(106.45)	(113.53)	(.0087)	(.0625)	(.0667)	(.0014)	(.0104)	(.0111)
Experience^2	B	-7.87	[-2.05]	[1.23]	-.0050	-.0031	[-.0010]	-.0007	-.0006	[-.0003]
	s.e.	(.29)	(1.70)	(1.85)	(.0002)	(.0010)	(.0011)	(.0000)	(.0002)	(.0002)
Education x Experience	B	+	[11.42]	[7.04]	[+]	[-.0002]	[-.0030]	-	[-.0009]	[-.0013]
	s.e.		(9.61)	(9.66)		(.0056)	(.0057)		(.0009)	(.0009)
Education x Experience^2	B	[-]	[-.30]	[-.21]	-	[-.0001]	[-.0001]	-	[-.0000]	[.0000]
	s.e.		(.16)	(.16)		(.0001)	(.0001)		(.0000)	(.0000)
Years of Higher Ed. x Exp.	B	+	[40.74]	47.92	+	[.0138]	[.0178]	+	[.0023]	[.0028]
	s.e.		(21.44)	(21.46)		(.0126)	(.0126)		(.0021)	(.0021)
Years of Higher Ed. x Exp.^{2}	B	+	[-.49]	[-.68]	+	[-.0001]	[-.0002]	+	[-.0000]	[-.0000]
	s.e.		(.48)	(.48)		(.0003)	(.0003)		(.0000)	(.0000)
BA x Experience	B	+	295.02	276.78	+	.1924	.1846	+	.0245	.0238
	s.e.		(98.20)	(98.15)		(.0577)	(.0577)		(.0096)	(.0096)
BA x Experience^2	B	+	-6.29	-5.84	+	-.0041	-.0040	+	-.0005	-.0005
	s.e.		(2.34)	(2.34)		(.0014)	(.0014)		(.0002)	(.0002)

(continued)

Table A1 continued

		Earnings			$Earnings^{1/3}$			Ln Earnings		
White	B	2334.70	2426.74	-2607.18	2.0821	2.1337	[-.0968]	.3528	.3601	[.1536]
	s.e.	(165.60)	(164.67)	(938.44)	(.0970)	(.0967)	(.5513)	(.0161)	(.0161)	(.0916)
'Other' Race	B	[697.46]	890.26	1389.49	.9677	1.0499	1.2074	.1773	.1869	.1937
	s.e.	(414.95)	(412.33)	(422.22)	(.2431)	(.2421)	(.2480)	(.0403)	(.0402)	(.0412)
White x Education	B	+	+	183.35	+	+	[.0530]	[+]	[+]	[-.0013]
	s.e.			(60.61)			(.0356)			(.0059)
White x Years Higher Ed.	B	+	+	[172.93]	+	+	[-.0044]	[+]	[+]	[-.0044]
	s.e.			(242.93)			(.1427)			(.0237)
White x BA	B	+	+	[791.20]	+	+	[.8337]	[+]	[+]	[.1383]
	s.e.			(1250.34)			(.7345)			(.1220)
White x Experience	B	[-]	[-]	265.39	[-]	[-]	.1569	[-]	[-]	.0223
	s.e.			(54.98)			(.0323)			(.0054)
White x $Experience^2$	B	-	[-]	-4.55	-	[-]	-.0029	-	[-]	-.0004
	s.e.			(1.05)			(.0006)			(.0001)
Constant		-5005.58	-2183.07	1817.46	9.5771	8.8230	10.5465	7.2796	6.9386	7.0869
R^2		.1984	.2095	.2112	.2320	.2391	.2402	.1936	.1984	.1991
S.D. of Resid.		6623.61	6578.39	6571.97	3.9805	3.8629	3.8606	.6434	.6416	.6414

Table A2

REGRESSIONS OF OCCUPATION ON BACKGROUND CHARACTERISTICS OF MEN AGED 25-64 WITH COMPLETE DATA AND POSITIVE EARNINGS, BY RACE

		Occupation			
		OCG		PSID	
		White (N=10,325)	Non-White (N=1110)	White (N=1260)	Non-White (N=514)
Father's Education	B	.8322	.8518	1.1394	1.2526
	s.e.	(.0647)	(.1590)	(.2025)	(.3023)
Father's Occupation	B	.2187	[.0965]	[.0229]	[-.0746]
	s.e.	(.0177)	(.0662)	(.0701)	(.1005)
Father White Collar	B	5.0016	[4.4099]	5.9318	18.4689
	s.e.	(.8212)	(2.9574)	(2.7026)	(4.9544)
Father Absent	B	-4.3337	-5.0803	[-1.1852]	-12.2798
	s.e.	(.6336)	(1.1816)	(7.1772)	(4.6494)
Non-South Upbringing	B	[.4386]	8.0662	[.9152]	[3.3944]
	s.e.	(.5129)	(1.1030)	(1.2963)	(1.8285)
Non-Farm Upbringing	B	5.5757	2.1284	3.5085	[o.6469]
	s.e.	(.5727)	(1.2341)	(1.4168)	(1.6874)
Siblings	B	-1.0463	-.3754	-.9934	[-.0671]
	s.e.	(.0750)	(.1564)	(.2233)	(.3019)
Father's Education^2	B	.0410	[.0293]	[-.0343]	.1022
	s.e.	(.0106)	(.0253)	(.0395)	(.0473)
Father's Occupation^2	B	-.0013	-.0074	[-.0035]	-.0094
	s.e.	(.0005)	(.0015)	(.0018)	(.0025)
Siblings^2	B	.0749	[.0164]	[.1075]	[.0742]
	s.e.	(.0184)	(.0304)	(.0785)	(.1086)
Constant		1.2518	-10.1246	.8920	-7.2736
R^2		.2049	.1956	.1319	.2425
S.D. of Residuals		22.0723	16.3622	19.5311	16.6410

Table A1

REGRESSIONS OF OCCUPATION ON BACKGROUND CHARACTERISTICS, EDUCATION AND EXPERIENCE OF MEN AGED 25-64 WITH COMPLETE DATA AND POSITIVE EARNINGS, BY RACE

		Occupation			
		OCG		PSID	
		White (N=10,395)	Non-White (N=1110)	White (N=1260)	Non-White (N=514)
Father's Education	B	.1172	.3919	[.3073]	[-.0232]
	s.e.	(.0566)	(.1393)	(.1723)	(.2526)
Father's Occupation	B	.1161	[.0424]	[.0561]	[.0366]
	s.e.	(.0151)	(.0557)	(.0576)	(.0800)
Father White Collar	B	[.8104]	[.7488]	[-.7733]	[4.7110]
	s.e.	(.6994)	(2.5051)	(2.2345)	(3.9943)
Father Absent	B	-1.3437	-2.0679	[1.0046]	[-2.5173]
		(.5405)	(1.0181)	(5.8854)	(3.7050)
Non-South Upbringing	B	-1.6743	5.8636	[-.4917]	[1.1010]
	s.e.	(.4387)	(.9416)	(1.0797)	(1.4891)
Non-Farm Upbringing	B	4.9025	[-.1079]	2.3937	[2.2341]
	s.e.	(.4875)	(1.0713)	(1.1698)	(1.3470)
Siblings	B	-.3310	[-.2000]	[-.0624]	[.2919]
	s.e.	(.0660)	(.1322)	(.1885)	(.2364)
Father's Education^2	B	[.0069]	[.0234]	[-.0569]	[.0178]
	s.e.	(.0091)	(.0214)	(.0326)	(.0383)
Father's Occupation^2	B	-.0016	-.0051	[-.0022]	-.0076
	s.e.	(.0004)	(.0013)	(.0015)	(.0020)
Siblings^2	B	[.0148]	[.0171]	[.0056]	[-.0833]
	s.e.	(.0156)	(.0256)	(.0651)	(.0851)
Years of Education	B	2.6852	.7863	1.6246	1.3412
	s.e.	(.0949)	(.1512)	(.2966)	(.2677)
Years of Education Past High School	B	2.3922	3.3765	3.1972	2.3216
	s.e.	(.3005)	(.8623)	(.6966)	(1.1113)
BA	B	5.3254	21.1203	5.4112	19.6487
	s.e.	(1.2832)	(3.9919)	(2.5616)	(5.4110)
Experience	B	.2081	.0880	[.0002]	[-.0650]
	s.e.	(.0178)	(.0425)	(.0435)	(.0624)
Experience^2	B	[.0015]	[-.0019]	[-.0001]	[.0002]
	s.e.	(.0013)	(.0030)	(.0032)	(.0043)
Constant		.8914	-8.0137	.6038	-4.5527
R^2		.4294	.4339	.4195	.5497
S.D. of Residuals		18.7035	13.7577	16.0036	12.8948

Chapter 9

The Determinants of Family Income

by Joseph Schwartz

This chapter will examine determinants of a family's total income, using data from the Panel Study of Income Dynamics (PSID, described in Appendix D). I hope to answer several questions about family income:

1. What is the relationship between Male Earnings (the factor examined in depth in previous chapters), other sources of family income, and total family income?
2. How does the inequality of family income relate to the inequality of its components, and how do transfers affect inequality?
3. What is the effect of family composition and changes in family composition on total family income?
4. What is the relationship between exogenous variables (family background, human capital, attitudinal, and community variables) and family income?

The cross-sectional analyses in this chapter will be based on data from 1972, the fifth year of the PSID. The chapter will conclude with a comparison of analyses based on annual data and those based on the average of several years of data.

Sample

I have chosen to conceptualize the family as having a principal male adult and/or a principal female adult, plus children under 18. I will ignore other adults. Where possible, I will also ignore the distinction between the "head" and the other adult family member. Unfortunately, this is not always possible. The PSID sampled households, not individuals, and the questionnaire only asked for detailed information about the "household head." It asked for considerably less information about other family members. The PSID questionnaire defines the "head" as the principal male adult whenever a male adult is present. This means that the PSID usually provides far more data on the principal male adult than on the principal female adult.

I eliminated all families in which the "head" was under 25, over 64, a student, or in the military in 1971. If the head changed between 1971 and 1972, I had no way of knowing whether the head had been a student or in the military in 1971, so I eliminated those who were students or in the military in 1972. In contrast to previous analyses in this book, I retained earners with zero or negative income and households with no principal male. These restrictions left me with 3,495 families, who constitute the total cross-sectional sample.

Certain analyses presented in this chapter will be performed on subsamples of these families. Depending on the issue being examined, it may or may not be appropriate to include in the same analysis families with different adult membership or different numbers of earners or, in the case of longitudinal analyses, families whose composition changed over the time period under examination. In particular, some cross-sectional analyses use samples restricted to families with two adult members or to families with two working adult members. Most of the analyses I will call "longitudinal" use a sample restricted to families with no missing income data and no change in adult membership for the years 1968-1972. This constitutes the "stable family" sample. I could have greatly increased the comparability across analyses by restricting the sample throughout to two-adult families where husband and wife both worked and lived together during the whole period 1968-1972, but only 22 percent of U.S. families had these characteristics. I have adopted the alternative of including all those families with "heads" aged 25-64 for whom any given analysis is logically appropriate, in order to give my conclusions as much generality as possible.

I define family income as the sum of five separate components.

1. Male Earnings is the principal male's income from wages, salary, and self-employment. If no male was present or if he did not work, this

component is zero.

2. Female Earnings is the principal female's income from wages, salary, and self-employment. Again, if no female was present or if she did not work, this component is zero.
3. Asset Income is the combined income of all family members from interest, dividends, and rent.
4. Welfare is the family's income from Aid to Families with Dependent Children, Aid to Dependent Children, and payments by the Welfare Department for items such as clothing, furniture, or rent.
5. Other Transfers is the family's income from unemployment benefits, Social Security, retirement and other pensions, alimony, and money received from relatives or friends.

I will call the sum of the first three components the family's "Taxable Income," although this may not correspond exactly to the family's taxable income as defined by the Internal Revenue code. Total Transfer Income is the sum of Welfare and Other Transfers.[1] This investigation will ignore other sources of family income, such as earnings of other family members. I have deflated all income figures to 1967 dollars. This was necessary for the longitudinal analyses appearing in the later sections of this chapter. The deflation factor for 1971 income variables was 1.213.

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[1] Asset Income is operationally defined as the difference between Taxable Income and the sum of Male and Female Earnings. Similarly, Other Transfer Income is the difference between Total Transfer Income and Welfare. In those cases in which the Institute for Social Research (ISR) made assignments to Male Earnings, Female Earnings, Taxable Income, Total Transfer Income, or Total Family Income (i.e. where Accuracy Codes were not 0), I treated the data as missing.

Family Income and Its Components

Table 9.1 shows the distribution of family income and its components. The families in my sample received about 72 percent of their total income from Male Earnings, 19 percent from Female Earnings, 4 percent from Assets, and 5 percent from Transfers. But few if any specific families have income composed in these proportions. If a family receives 90 percent of its income from earnings, for example, it is unlikely to get an additional 5 percent of its income from transfers.

Panel A of Table 9.2 shows the correlations between components of family income. Three points are notable.

1. There is a positive relationship (r = 0.21) between Male Earnings and Asset Income.
2. There is a negative relationship (r = -0.14) between Male Earnings and Female Earnings. This may seem somewhat surprising. Among two-adult families where both adults worked the correlation is 0.12. But if Male Earnings were high, the female was slightly less likely to work. Thus if one takes two-adult families as a whole, the correlation between Male and Female Earnings is -0.02. Furthermore, if the male was absent his earnings are by definition zero. Female Earnings tended to be above average in these circumstances, since females with no husband are more likely to work. This makes the overall correlation between Male and Female Earnings for all families, including those with one adult, slightly negative.
3. Both types of Transfer Income are negatively related to both sources of Earnings. This relationship is strongest between Total Transfer Income and Male Earnings (r = -0.33).

Other things being equal, a dollar increase in any component of income always leads to a dollar increase in total income. Thus if we regress Family

Table 9.1

Cumulative Frequency Distributions of the Components of 1971 Family Income (in 1967 Dollars) for Basic Sample of 3495 Cases in Which Head Was 25-64 Years Old, Not a Student, and Not in the Military in 1971.

	Male Earnings 1	Female Earnings 2	Asset Income 3	Taxable Income (Col. 1 + Col. 2 + Col. 3) 4	Welfare 5	Other Transfer Income 6	Total Transfer Income (Col. 5 + Col. 6) 7	Family Income (Col. 4 + Col. 7) 8
less than 0	.1			.1				.1
0*	24.6	46.8	55.6	5.4	94.1	71.9	68.8	.2
1-100	24.8	48.7	69.6	6.0	94.3	74.0	70.7	.3
100-200	25.0	50.1	74.8	6.2	94.5	75.8	72.3	.3
200-400	25.2	52.5	80.9	7.2	94.8	79.0	75.1	.5
400-1000	26.3	59.5	89.3	10.0	96.0	86.3	82.6	2.1
1000-1500	27.2	63.1	91.9	11.7	97.1	90.1	87.0	3.9
1500-2000	28.2	66.6	94.2	13.8	98.0	92.4	89.9	6.2
2000-3000	30.5	73.6	96.4	18.0	99.1	96.3	95.0	12.0
3000-4000	34.2	80.7	97.5	23.1	99.7	98.0	97.5	18.6
4000-5000	39.5	86.5	98.1	28.3	99.9	99.1	98.9	24.5
5000-6000	45.5	90.8	98.3	33.9	100.0	99.3	99.3	30.3
6000-7000	52.5	94.5	98.6	41.1		99.5	99.5	37.9
7000-8000	59.7	96.7	98.6	47.0		99.8	99.8	44.6
8000-9000	66.3	97.9	98.7	53.3		99.9	99.8	51.1
9000-10,000	74.7	99.0	98.8	61.5		100.0	100.0	59.7
10,000-12,000	83.2	99.6	98.9	72.6				71.3
12,000-15,000	91.7	100.0	99.1	84.8				83.9
15,000-20,000	96.1		99.6	93.9				93.7
20,000-25,000	98.2		99.7	96.9				96.9
25,000-35,000	99.4		99.9	98.8				98.8
35,000-45,000	99.8		100.0	99.6				99.6
over 45,000	100.0		100.0	100.0				100.0
Mean	7018.62	1823.46	368.37	9200.80	103.48	424.74	528.22	9729.02
Standard Deviation	6880.96	2616.54	1217.49	7356.09	509.95	1068.66	1179.04	7051.56
N	3478	3460	3441	3441	3495	3477	3477	3425
Minimum	-7909.32	0	0	-5770.82	0	0	0	-5770.32
Maximum	82439.44	20610.07	47921.52	82439.44	6252.27	9816.16	9816.16	82439.44
Percent of Total Family Income From this Source	72.1	18.7	3.8	94.6	1.1	4.4	5.4	100.0

*"0" means that the family received no income of this type. For earnings, a "0" might indicate

Table 9.2

A. Correlation Matrix for Components of 1971 Family Income (1967 dollars) and Unstandardized Bivariate Regression Coefficients(B) Predicting Family Income from Each Component for 3160 Households with Complete Data on All Basic Variables **(see Table 9.8)**and with a Non-student, Non-military Head Aged 25-64.

	Male Earnings	Female Earnings	Assets	Welfare	Other Transfers	Total Taxable Income	Total Transfers	Total Family Income
Male Earnings	1.00000							
Female Earnings	-.14062	1.00000						
Assets	.21095	.02347	1.00000					
Welfare	-.19827	-.11020	-.04534	1.00000				
Other Trans. Inc.	-.26404	-.06067	.05007	-.00910	1.00000			
Taxable Income	.92115	.22739	.34574	-.23197	-.26027	1.00000		
All Transfers	-.32569	-.10353	.02494	.43581	.89603	-.33726	1.00000	
Total Family Income	.90750	.22020	.36489	-.17018	-.12380	.98787	-.18699	1.00000
B	.928	.595	2.151	-2.324	-.834	.947	-1.134	1.000

B. Variance-Covariance Matrix for Components of 1971 Family Income (1967 dollars) for 3160 Households with Complete Data on All Basic Variables (see Table 9.8) and with a Non-student, Non-military Head Aged 25-64.

	Male Earnings	Female Earnings	Assets	Welfare	Other Transfers	Total Taxable Income	Total Transfers	Total Family Income
Male Earnings	47494505							
Female Earnings	-2525385	6790746						
Assets	1737853	73111	1428972					
Welfare	-705165	-148197	-27972	266328				
Other Trans. Inc.	-1903642	-165397	62616	-4915	1094429			
Taxable Income	46679718	4357188	3039049	-880272	-2002138	54069502		
All Transfers	-2608802	-313574	34652	261411	1089510	-2882412	1350922	
Total Family Income	44070937	4043524	3073674	-618856	-912638	51187032	-1531501	49655567

Note: Numbers in the diagonal are variances. The variance of Male Earnings is thus ($6892)^2 = 47,494,505. Numbers off the diagonal are covariances, where Cov(X,Y) = $s_x s_y r_{xy}$.

Note that the variances are not identical with the square of the standard deviations in Table 1 due to deletion of/missing data on all basic variables.
respondents with

Income on all its components simultaneously, the regression coefficient of each component is necessarily 1.000. But other things are rarely equal, so the bivariate regression coefficients are seldom 1.000. A dollar advantage in earnings, for example, is usually associated with a disadvantage in transfers. The net increase in family income is thus less than a dollar. The bivariate regression coefficient of Family Income (Y) on one of its components (C) is equal to Cov (Y,C)/Var (C). These regression coefficients are presented in the last row of Panel A. A $1.00 increase in Male Earnings is associated with a $0.93 increase in total Family Income, while a $1.00 increase in Asset Income is associated with a $2.15 increase in total Family Income. A $1.00 increase in Transfer Income, in contrast, is associated with a $1.13 decrease in total Family Income, implying a decrease in Taxable Income of $2.13.

Tables 9.1 and 9.2 lump together families with male and female heads and with zero, one, or two employed adults. Analogous tables for two-adult families and two-working-adult families appear in the appendix. Except for the relationship between Male and Female Earnings, the variance-covariance matrices are quite similar for all subsamples. As mentioned above, when the sample is restricted to two-adult families, the negative relationship between Male and Female Earnings becomes one of almost total independence (r = -.02). Restricting the sample to two-adult families where both adults worked creates a moderate positive relationship between the two earnings components (r = .12). In this last sample, all the components of Taxable Income are positively intercorrelated. For this reason, each of these components has a bivariate regression coefficient greater than 1.000 when predicting Family Income. The selection of the sample thus has an important substantive impact on later results.

In order to see what these interrelationships imply for overall inequality in family income, I decomposed the variance of family income into the variance of income from each separate source and the covariance between different sources. Suppose, for example, that we separate family income into Taxable Income and Transfer Income. Then the following formula can be written:

$$\text{Var (Family Income)} = \text{Var (Taxable Income)} + \text{Var (Transfer Income)} + 2\ \text{Cov (Taxable Income, Transfer Income)}$$

where Var is the variance and Cov is the covariance. Panel B of Table 9.2 presents the variance-covariance matrix of the components of family income for the sample of all households. The above formula can be empirically verified by substitution from the table. Thus the variance of taxable income is 54.1 million, the variance of transfer income is 1.4 million, the covariance is -2.9 million, and $54.1 + 1.4 + 2\ (-2.9) = 49.7$, which is the total variance of family income. This means that the present distribution of transfers makes the variance of family income 8.1 percent less than the variance of taxable income (49.7 compared with 54.1 million). This corresponds to a difference between the standard deviations of family income and taxable income of $1 - (1-.081)^{1/2} = 4.14$ percent. The combined effect of this reduction in the standard deviation and a 5.74 (528.22/9200.80) percent increase in the mean is a reduction in the coefficient of variation of $100-(100 - 4.14)/(100 + 5.74) = 9.1$ percent.

The effect of transfers on inequality depends not only on how they are distributed but on how they are collected. Consider the case in which the income for transfers is collected by a flat-rate tax on all earnings and asset income. (Pechman and Okner conclude that the United States "tax system is virtually proportional for the vast majority of families". 1974:64). To raise an average of 528.22 dollars per family, the tax rate in this population would have to be 5.74 percent. Such a proportional tax would reduce both the mean and the standard deviation of taxable income by 5.74 percent. The variance of after-tax, pre-transfer income would thus be $(\$54,069,502)(1 - 0.0574)^2 = 48$ million. The variance of transfers is 1.4 million. Since the correlation of transfers with taxable income is -0.337, the covariance is $(-\ 0.337)(48.0)^{\frac{1}{2}}(1.4)^{\frac{1}{2}} = -\ 2.7$ million. The variance of family income after transfers would therefore be 48.0 + 1.4 + (2) (- 2.7) = 44.0 million. If taxes are roughly proportional, then, the addition of transfers and the deduction of taxes to pay for these transfers would reduce the standard deviation of taxable income by $100-(44.0/54.1)^{\frac{1}{2}}$ = 9.8 percent. Ignoring administrative costs and possible effects on work effort, the payment of transfers and the collection of taxes leaves the mean unchanged. The coefficient of variation (V) for post-tax family income is therefore reduced by the same 9.8 percent. This suggests that the existing distribution of transfer income reduces inequality by about 10 percent.

Would an alternative distribution lead to greater reductions? Suppose that all of the transfer income were distributed to the poorest families. This would, in effect, set a floor on the family income distribution and all families with taxable incomes below this level would receive the difference in transfers. (This would be a "guaranteed family income" policy.) In geometric terms, a plot of the relationship between taxable income and transfer income would have a slope of -1.0 for income levels below the minimum and a slope of zero elsewhere. The exact level of the floor depends on both the aggregate amount of income to be transferred and the distribution of taxable income.

If the \$528.22 x 3441 in aggregate transfer payments were distributed only to the poorest families in the PSID sample, the cut-off point would be \$3840. The 771/3441=22.4 percent of the sample with incomes below \$3840 have a mean taxable income of \$1482. Each family would receive an average transfer payment of (\$528)(3441/771)= \$2358, raising the average family income to \$3840. If all the transfer income were distributed in this manner, the post-transfer pre-tax income distribution would have a standard deviation of \$6820. This would be 7.3 percent less than the standard deviation of taxable income and 3.2 percent less than the standard deviation of the observed family income distribution. The net effect of a 7.3

percent decrease in the standard deviation and a 5.7 percent increase in the mean is a 100 - 92.7/105.7 = 12.3 percent decrease in the coefficient of variation. I conclude that this maximally egalitarian distribution of transfer income would reduce the standard deviation and coefficient of variation of taxable income by 3.2 percent more than does the observed distribution. It would produce a more radical change in measures of inequality that are more sensitive than the coefficient of variation to changes at the bottom of the income distribution.

As a final exercise I wanted to know what would happen if the money to be distributed as transfer income were collected by taxing only the highest incomes. A 100 percent tax rate on all income over $19,140 would raise an amount equal to all transfer income. The combined effect of this tax/transfer policy can be seen from Table 9.3. The within-cell variance of the poorest and the richest groups has been eliminated and the means of the two groups have moved towards the overall mean. Since the overall mean remains constant, the coefficient of variation as well as the standard deviation is reduced by 29.5 percent. This demonstrates that the collection of taxes can have a greater effect on inequality (as measured by the coefficient of variation) than does the distribution of transfers.

One can also use the decomposition of variance to explore the relationships among components of taxable income. Given that Male Earnings is the largest source of income, I wanted to know how the distributions of Female Earnings and Asset Income affect inequality. The variance of Female Earnings + Male Earnings is $(47.5 + 6.8 + 2\ (-\ 2.5)) = 47.5 \times (1.018)^2$. The standard deviation of this sum is thus 1.8 percent greater than the standard deviation of Male Earnings. The mean of the sum is 25.5 percent greater, making the coefficient of variation 100 - (101.8/125.5) = 18.9 percent smaller. Analogous calculations show the coefficient of variation of Male Earnings + Asset Income to be 0.1 percent less than that of Male Earnings alone. These results suggest that the addition of

Table 9.3

Effect of a Hypothetical Transfer and Tax Policy which Redistributes 5.74 percent of Gross Taxable Income from the Richest to the Poorest Families.

Subgroup with Taxable Income and transfers	N	Taxable Income before taxes and transfers within subgroup		Net Income after taxes and transfers within subgroup	
		mean	s.d.	mean	s.d.
less than $3840	771	1482	3840	3840	0
$3840 to $19140	2434	9935	3728	9935	3728
more than $19140	236	26842	11344	19140	0
Total	3441	9201	7356	9201	5183

Female Earnings reduces overall inequality while the addition of Asset Income has a negligible effect.[2]

The decomposition of variance is one approach to the investigation of sources of inequality in family income. I now turn to a somewhat different approach, comparing the inequality of family income and the inequality of its components. I will use four measures of inequality: the coefficient of variation (V), the standard deviation of the natural logarithm (s_{ln}), Atkinson's measure (A) ., and the standard deviation of the cube root of income ($s_{1/3}$).[3] Table 4 presents these measures for Male and Female Earnings, Taxable Income, and total Family Income for several subsamples.

The most striking observation is that according to every measure of inequality, Family Income is more equally distributed than any of its major components, that is, Male Earnings, Female Earnings, or Taxable Income. This is consistent with the result obtained earlier that Transfer Income has an equalizing effect on Family Income. Whereas the effect on the coefficient of variation is quite modest, the effect on measures sensitive to changes in the lower range of the income scale (most noticeably, s_{ln}) is much greater. The information in the table also supports the hypothesis that as the level of aggregation increases from individual earnings to taxable income to family income, the level of inequality decreases.

One also observes that as the homogeneity of the sample increases (with re-

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[2] If one examines two-adult families using Appendix Tables A2 and A3, the equalizing effect of Female Earnings is somewhat diminished but Asset Income slightly increases (by 1-2 percent) the inequality in Male Earnings.

[3] The last inequality measure is not scale invariant. Introduced in another paper (Schwartz, unpublished), this measure is based on the assumption that the utility of income is linearly related to its cube root. In a comparison of the distribution of total Family Income to the distribution of Earnings (or one of its other major components), this measure will remain constant if the distribution of all other sources of income is such that the marginal utility of this other income is uncorrelated with the utility of earnings. (cont on p. 9a)

3/ (continued)

Atkinson's (1972) measure is also computed on the assumption that utility is a linear function of Income $^{1/3}$. It estimates the "inefficiency" of the present distribution of income compared to a completely equal distribution of the same sum in generating social welfare. If utility is a linear function of $Y^{1/3}$, it would take $(\overline{Y^{1/3}})^3$ dollars per person to generate the present level of social welfare if this sum were distributed equally. We currently spend $\overline{Y}$ dollars per person to generate the same level of welfare. Atkinson's index is thus:

$$A = 1 - \frac{(\overline{Y^{1/3}})^3}{\overline{Y}}$$

Table 9.4

Measures of Inequality for Components of 1971 Income (1967 dollars) for Subsamples, Each Having Complete Data on All Basic variables and With a Non-student, Non-military Head Aged 25-64*

		Male Wages	Male Hours	Male Earnings	Female Wages	Female Hours	Female Earnings	Taxable Income	Total Family Income
A. All Households N=3160	V							.796	.723
	$s_{\ln}$							2.215	.897
	A							.257	.151
	$s_{1/3}$							6.918	4.986
B. All Households With An Employed Male N=2391	V	.941	.307	.690				.648	.626
	$s_{\ln}$	.618	.481	.869				.791	.702
	A			.130				.119	.106
	$s_{1/3}$			4.579				4.635	4.371
C. All Households With An Employed Female N=1661	V				.805	.554	.788	.656	.605
	$s_{\ln}$				.663	1.000	1.314	.951	.700
	A						.243	.158	.120
	$s_{1/3}$						4.600	5.162	4.444
D. All Married Couples N-2245	V		.363	.723		1.275	1.474	.661	.622
	$s_{\ln}$		1.540	1.872		3.489	3.870	1.348	.699
	A			.188			.800	.145	.104
	$s_{1/3}$			5.860			7.368	5.331	4.349
E. All Married Couples Where Both Work N-1134	V	.876	.285	.559	.723	.599	.806	.492	.476
	$s_{\ln}$	.571	.440	.691	.644	1.065	1.388	.559	.497
	A			.097			.259	.077	.070
	$s_{1/3}$			3.820			4.611	3.764	3.570
F1. All Stable (1968-72) Households 1971 Income N=2005	V							.718	.653
	$s_{\ln}$							2.117	.812
	A							.229	.131
	$s_{1/3}$							6.651	4.726
F2. All Stable Households 1968-1971 Average Income N=2005	V							.651	.597
	$s_{\ln}$							1.748	.663
	A							.185	.111
	$s_{1/3}$							5.819	4.209
All Married Couples Without 2 Working Adults (D-E) N=1111	V		.433	.832		6.970	7.195	.833	.768
	$s_{\ln}$		2.105	2.534		1.179	1.243	1.786	.836
	A			.269			.999	.210	.136
	$s_{1/3}$			7.365			2.202	6.340	4.891
All One-Adult Households (A-D) N=915	V							.987	.754
	$s_{\ln}$							3.061	.984
	A							.438	.169
	$s_{1/3}$							7.272	4.269
All Households Without 2 Working Adults (A-E) N=2026	V							.993	.871
	$s_{\ln}$							2.605	.988
	A							.341	.182
	$s_{1/3}$							7.523	5.168

*The log of 0 has been set to zero. Sample E regrettably includes six women who worked without pay. The inclusion of women with no income in the computation of $S_{\ln}$ makes comparisons with other samples meaningless.

spect to employment and marital status) each particular distribution grows more equal. The bottom portion of Table 9.4 shows measures of inequality for those subsamples which were excluded by successive restrictions. It is clear that these excluded subsamples exhibit much more within-group inequality than the included samples.

The remainder of this section will examine inequality in Earnings, the largest component of Family Income and also the largest source of variation in such income.

Male Earnings are more equally distributed than Female Earnings, even when we compare only males and females who worked. This appears to be because Female Hours vary more than Male Hours. The inequality measures for Male and Female Wages are fairly similar. Indeed, V is larger for Male Wages than Female Wages, though s_{ln} is not. Since V primarily reflects inequality at the upper end of the distribution, while s_{ln} is most sensitive to inequality at the bottom, it appears that inequality among women is primarily a matter of some women earning far less than the average woman (i.e., skewed to the left), while inequality among men is primarily a matter of some men earning far more than average (i.e., skewed to the right).

Inequality in Earnings is a function of the inequality in its two components, wages and hours. We know that Ln Earnings = Ln Wages + Ln Hours, so Var (Ln Earnings) = Var (Ln Wages) + Var (Ln Hours) + 2 Cov (Ln Wages, Ln Hours). Since Var (Ln Wages) is greater than Var (Ln Hours) for employed males, inequality in Male Earnings primarily reflects inequality of wages. S_{ln} and V disagree on whether women's wages are more equal than their Hours Worked. I conclude that those women with high wages and those with low Hours Worked make the greatest contribution to inequality of Female Earnings.

The relationship between inequality in wages, hours, and total earnings can also be seen by examining the variance-covariance matrices of Ln Wages, Hours, and Earnings in Table 9.5. This matrix shows that wages and hours are relatively

Table 9.5

Variance-Covariance Matrices for Components of 1971 Male Ln Earnings and 1971 Female Ln Earnings for 1134 Households With an Employed Non-student, Non-military Male Head Aged 25-64, Employed Spouse, and Complete Data on All Basic Variables.

	Ln Male Wages	Ln Male Hours	Ln Male Earnings
Ln Male Wages	.3263		
Ln Male Hours	-.0215	.1938	
Ln Male Earnings	.3048	.1723	.4771

	Ln Female Wages	Ln Female Hours	Ln Female Earnings
Ln Female Wages	.4142		
Ln Female Hours	.0499	1.1230	
Ln Female Earnings	.4641	1.1729	1.6370

Note: The matrix of female variables has been adjusted to remove the effect of six women who had zero earnings (despite working a positive number of hours). This adjustment resulted in a substantial reduction in the variance of Ln Female Earnings (compare to s_{ln} in Table 4) and minor reductions in the other entries.

independent of one another. The covariance of Ln Hours and Ln Wages is consistently small (- 0.0215 for males, 0.0499 for females). The variance of Ln Earnings is therefore almost equal to the sum of the variances of Ln Wages and Ln hours. Male Wages account for approximately 63 percent of the inequality in Male Earnings, while Female Hours account for about 72 percent of the inequality in Female Earnings.

Family Composition and Family Income

One obvious source of income differences between families is their composition. Family composition is an indicator of potential sources of income. For example, single-adult families can receive earnings from only one source; families without children do not receive AFDC; women with young children are likely to work fewer hours than other women, or not to work at all. Knowing a family's potential sources of income is the first step in predicting how much income it receives. Considerably more is learned by examining whether or not the principal adults worked during the year. The cross-classification of family composition by employment status of the principal adults formed the basis of the family typology used to explore the relationship between these factors and the components of family income. Table 9.6 shows the extent to which different types of families depend on different sources of income, and the proportion of variance in each kind of income explained by family composition.[4]

This relationship is important for several reasons. First, it explains a considerable proportion of the variance in all sources of income except assets. Second, many of the remaining analyses in this chapter will exclude exactly those

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[4] It is reassuring that those families with only one adult received only one type of earnings. However, it is disconcerting that families occasionally received earnings from members who claimed they worked zero hours during the year. Since all cases with missing data on Earnings or Hours Worked have been excluded, there is no obvious explanation. Fortunately the magnitude of the problem appears small.

Table 9.6

Mean 1971 Income (1967 dollars) by Source and Family Type: 3341 Households with Complete Data on Each Income Source and on Hours Worked by Principal Male and Female Adults and with a Non-student, Non-military Head Aged 25-64.

Male Adult	Female Adult	Children Aged ≤10			Male Earnings 1	Female Earnings 2	Asset Income 3	Taxable Income (Cols 1+2+3) 4	Welfare 5	Other Transfer Income 6	Total Trans.Inc. (Cols 5+6) 7	Family Income (Cols 4+7) 8
A	U	A*		Mean	0	12	376	388	444	1329	1772	2160
			N=106	S.D.	0	90	1002	1005	754	1694	1542	1912
				% of Family Income	0	.55	17.39	17.94	20.54	61.52	82.06	
A	U	P		Mean	0	40	74	114	1517	996	2513	2627
			N=67	S.D.	0	169	241	292	1437	1295	1264	1274
				% of Family Income	0	1.53	2.81	4.34	57.74	37.92	95.66	
A	E	A		Mean	0	4576	330	4906	63	506	569	5475
			N=408	S.D.	0	3087	920	3398	375	1085	1128	3429
				% of Family Income	0	83.59	6.03	89.62	1.15	9.24	10.38	
A	E	P		Mean	0	3319	72	3391	485	904	1389	4780
			N=128	S.D.	0	2594	332	2614	1040	1175	1460	2421
				% of Family Income	0	69.44	1.50	70.94	10.15	18.91	29.06	
U	A	A		Mean	0	0	155	155	228	2191	2419	2574
			N=25	S.D.	0	0	346	346	631	1331	1090	1298
				% of Family Income	0	0	6.00	6.00	8.86	85.14	94.00	
U	A	P		Mean	0	0	0	0	898	3427	4325	4325
			N=2	S.D.	0	0	0	0	1131	4178	3899	3899
				% of Family Income	0	0	0	0	20.76	79.24	100.00	
U	U	A		Mean	242	0	854	1096	178	3465	3644	4740
			N=46	S.D.	1101	0	1916	2112	466	2683	2542	3063
				% of Family Income	5.11	0	18.02	23.13	3.77	73.10	76.87	
U	U	P		Mean	76	0	26	102	1640	1834	3474	3575
			N=10	S.D.	418	0	151	439	2134	1697	1464	1430
				% of Family Income	2.12	0	.73	2.85	45.86	51.29	97.15	
U	E	A		Mean	95	2948	1043	4086	152	2613	2764	6851
			N=30	S.D.	864	2164	1619	3493	391	1960	1890	3364
				% of Family Income	1.39	43.03	15.23	59.65	2.21	38.14	40.35	
U	E	P		Mean	0	1674	980	2653	254	2926	3180	5834
			N=6	S.D.	0	1405	1529	2625	636	1325	1026	2981
				% of Family Income	0	28.69	16.80	45.49	4.36	50.15	54.51	
E	A	A		Mean	7363	0	419	7637	31	189	220	7857
			N=225	S.D.	5097	0	1785	5347	218	526	559	5227
				% of Family Income	93.71	0	5.33	97.20	.39	2.41	2.80	
E	A	P		Mean	5457	0	63	5520	0	245	245	5765
			N=21	S.D.	2715	0	110	2764	0	520	520	2540
				% of Family Income	94.66	0	1.09	95.76	0	4.24	4.24	

Table 9.6 continued

		1	2	3	4	5	6	7	8
E U A N=499	Mean	10872	0	648	11520	13	323	336	11856
	S.D.	9256	0	1605	9973	139	924	937	9880
	% of Family Income	91.70	0	5.47	97.17	.11	2.72	2.83	
E U P N=578	Mean	10168	0	293	10462	67	137	204	10666
	S.D.	6448	0	1294	7079	480	537	755	6963
	% of Family Income	95.34	0	2.75	98.09	.62	1.29	1.91	
E E A N=629	Mean	8986	3547	423	12956	12	196	207	13163
	S.D.	5619	2592	1168	6856	183	623	647	6766
	% of Family Income	68.27	26.94	3.21	98.42	.08	1.49	1.57	
E E P N=562	Mean	8197	2613	191	11202	21	253	273	11475
	S.D.	3852	2274	728	4564	222	778	807	4520
	% of Family Income	73.18	22.77	1.66	97.62	.18	2.20	2.38	
Total Sample N=3343		7019	1823	368	9201	103	425	528	9729
		6881	2617	1217	7356	510	1069	1179	7052
		72.14	18.74	3.79	94.57	1.06	4.37	5.43	
Eta^2 when groups defined by:									
Adult Membership[a] d.f.=2		.2915	.1065	.0020	.1884	.0547	.0271	.0594	.1716
Family Membership[b] d.f.=5		.2918	.1302	.0154	.1943	.1365	.0342	.0912	.1773
Family Membership[b] and Employment Status of Adults d.f.=15		.3625	.4752	.0225	.2672	.2374	.1953	.3386	.2179

* A - absent
P - present
U - present and did not work during 1971
E - present and worked during 1971

a. Adult Membership is the presence or absence of a principal male and/or female adult.

b. Family Membership covers adult membership and the presence or absence of children less than 10 years old.

family types (with a single or non-working adult) that tend to have the lowest incomes and receive the highest transfers. It is important to understand how such restrictions affect the general character of the sample under examination. Finally, family composition is the most important factor in determining how much income a family "needs." Although I have not tried to estimate these needs and do not intend to do so in this chapter, it seems reasonable to assume that one-adult families without young children (primarily single individuals) need less money than two-adult families with children.

Some of the inter-relationships among the components of family income previously discussed become even clearer within the context of family composition. For example, single working women earn more than married working women (partially explaining the negative relationship between Male and Female Earnings); families with a working man receive very little welfare and have the highest average family incomes; and most of the Other Transfers (primarily unemployment and retirement benefits) go to families with a non-working male adult or no male adult at all. In two-adult families, husbands earn less on average when their wives work (causal order could be in either direction) but those families with both a working man and woman have higher total Earnings and Family Income. Working male adults in families with no working female earn over 90 percent of the total Family Income. When both spouses work, the husband earns about 70 percent and the wife about 25 percent of total Family Income. This implies that earlier chapters which examined earnings for men with positive earnings were focusing on the largest source of family income, although ignoring an important second source for families with working wives.

The values of eta^2 in Table 9.6 are considerably lower for Family Income than for Ln Family Income or Family $\text{Income}^{1/3}$. Adult membership (the presence or absence of principal male and female adults), for example, explains 17.16 percent of the variance in Family Income, 23.40 percent of the variance in Ln Family Income, and 25.81 percent of the variance in Family $\text{Income}^{1/3}$.

This trend also holds for Male Earnings and Taxable Income.

In theory, longitudinal data allow one to examine the effects of changes in family composition on the components of family income. However, 60 percent of families experienced no change over five years, while another 30 percent experienced no change in the principal adults of the family.

Still, it seemed useful to analyze the effect of those changes that did occur. For the sake of clarity I restricted the analysis to families whose composition did not change more than once during the five years. I defined the change in income as the difference between income received the year before the reported change and income received the year after it. This two-year difference was chosen in order to assure that there would be no change in family composition during the years the income was measured. If no change in composition was reported, the change in income was arbitrarily set equal to the difference between the incomes reported in the 1969 and 1971 interviews.

The results of this analysis appear in Table 9.7 Unfortunately they are not very interesting. Only the gain or loss of a husband had a large effect on changes in income, and this primarily affects Male Earnings. The other effects of changes in family composition on changes in income were small and almost always in an intuitively predictable direction. For example, the loss of a husband was associated with modest increases in the family's Female Earnings and Total Transfer Income, amounting to about 20 percent of the loss in Male Earnings. A second general result of this analysis is that the variance of the change in income within each group of families experiencing a change in composition was greater than the variance of the change in income of the group of non-changing families. This means that families with a stable composition tend to be more economically stable than families whose composition changes.

The change in income associated with each type of change in family composition is smaller than the difference between the average incomes of families of the pre-change and post-change types. For example, if we examine the 10 cases in which a woman married, Male Earnings increased from zero to an average of $7001. This is much less than the average married man's earnings of $9188. Similarly, while Female Earnings decrease, indicating that women

Table 9.7

Change in Income Over Two Years (1967 dollars) by **Changes** in **Family Composition for Families** With No **More** than One Change in **Composition** Between 1967 and 1972 **And With Income Data** for the Year Prior to and the **Year After** the Change in **Family Composition** and With a Non-military, Non-student Head Aged 25-64 in 1971.*

Type of Change:		N	**Male** Earnings	**Female** Earnings	**Tax able** Income	**Total Transfer** Income	**Total Family** Income
None (1970-1968 income)	mean	1227	149	128	315	182	497
	s.d.		2848	1477	3202	999	3081
Change in Members Other than Head or Wife	mean	547	593	-227	497	128	627
	s.d.		3316	1394	3635	560	3624
Change in Wife	mean	34	232	-1287	-1086	-2	-1088
	s.d.		2731	3395	4684	172	4673
Husband Gone	mean	79	-5640	544	-5227	651	-4586
	s.d.		4210	1924	4496	1593	4048
New Husband	mean	10	7001	-673	6628	-283	6345
	s.d.		3872	2170	3763	578	3810
Former "Non-Principal Adult" Now Head	mean	55	-1050	282	-1312	151	-1162
	s.d.		6376	3933	5641	699	5538
Female (not wife) Now Married	mean	7	1496	1292	2420	-373	2047
	s.d.		4307	3257	5274	1289	5062
Total Sample	mean	2011	54	26	126	177	302
	s.d.		3187	1670	3498	921	3398
Eta^2			.140	.027	.107	.014	.098
			10.71	7.03	30.11	3.66	27.22

Mean Income for Families of Each Type

Family's Adult Structure

Female Adult Only	709	0	3239	3505	1080	4585
Male Adult Only	273	6485	0	6730	453	7183
Two Adults	2361	9188	1609	11196	371	11568
Total	3343	7019	1823	9201	528	9729

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* For families with no change in composition between 1967 and 1972 change in income is difference between 1968 and 1970 income.

alter their economic behavior during the year following marriage, the change is only 41 percent of the difference between the average married woman and the average single woman. This is not a surprising result. Changes in a person's situation frequently require time to affect their behavior. Furthermore, a "just-married" woman may well differ from the "average" woman in many ways including age and number of children. The main conclusion is simply that families which change their composition are unlikely to be representative of either their pre- or post-change family type.

Bivariate Relationships

The next two sections will focus on the association between exogenous variables such as age and education and the endogenous components of family income. The variables to be included in this discussion are presented in Table 9.8. The means and standard deviations of these variables for four different samples are presented in Table 9.14 in the appendix.

Earlier chapters explored the relationship between the exogenous variables and Male Earnings. The aim of this section is to examine the extent to which these variables influence Family Income independently of their effects on Male Earnings.

The unstandardized regression coefficient of a single exogenous variable (such as husband's Education) when predicting Family Income equals the sum of the coefficients obtained when each component of income is regressed on this same exogenous variable. This means we can decompose the bivariate relationship between, for example, an adult male's education and his family's income into five other bivariate relationships. We can then see whether the relationship is entirely due to the effect of the male's education on his own earnings, or whether his education also

Table 9.8

Unstandardized Bivariate Regression Coefficient of Exogenous Variables When Predicting Each Source of 1971 Income (in 1967 dollars): 2245 Two-Adult Families With Complete Data on Basic Variables and Male Non-military, Non-student Head Aged 25-64

	Male Earnings[1]	Female Earnings[1]	Assets[1]	Welfare[1]	Other Transfers[1]	Taxable Income[1]	Total Transfers[1]	Total Income[1]
Head White	2597 **	-38	194	-143 **	36	2753 **	-107	2646 **
H's Father's Education (years)	445 **	64 *	17	-9 *	-9	525 ***	-18	508 ***
H's Father's Occupation (Duncan Score)	71 **	10 *	3	-1	-1	84 **	-3	81 **
H's Father White Collar	3161 **	550 *	195 *	-41	-111	3906 ***	-151	3755 ***
H's Father U.S. Citizen[1]	-1642 *	-16	-337 **	-49	-2	-1995 **	-51	-2046 **
H's Siblings[1]	-516 **	-60 *	-2	12 *	20	-578 **	32 *	-546 **
H's Non-Farm Origins[1]	2986 ***	243	112	-8	45	3341 ***	37	3378 ***
H's City Origins[1]	2934 **	116	219 *	-15	82	3268 **	67	3335 ***
H's Non-South Origins[1]	2426 **	-1	141	-1	-63	2567 **	-64	2503 **
H's Parents' Economic Situation[2]	619 **	14	46	-16 *	-27	679 **	-42	637 **
H's Non-School Training[1] (0,1)	-477	-30	-38	-19	173 *	-544	154 *	-390
Number of Children	170	-268 **	-62 *	35 **	-41 *	-160	-6	-166
Age Youngest Child (no children = 18)	-10	66 **	24 **	-3	21 **	80 *	19 **	99 *
H's Low Educational Goals for Children (1-5)	-320 *	-237 **	-75 *	28 **	-23	-632 **	5	-628 **
H's Achievement Motivation (0-16)	571 ***	54 *	31 *	- 9 *	-8	656 ***	-17	638 ***
H's Risk Avoidance (0-8)	1373 ***	190 **	129 **	-34 **	-16	1692 ***	-50 *	1642 ***
H Union Member[1]	-47	-15	-15 *	-1	-6	-77 *	-7	-84 *
H's Job Tenure[1] (months)	10 **	-1	1	-0 *	-1 **	10 **	-2 **	8 **
H Self-Employed[1,3]	1328 **	-252 *	200 **	-3	-66	1276 **	-69	1207 **
H Physically Handicapped	-3591 **	-142	64	125 **	711 ***	-3669 **	836 ***	-2832 **
Local Shortage of Female Labor (0-7)	-25	-8	23	9	14	-9	23	14
Local Shortage of Male Labor (1-5)	162	-19	39	13	-0	182	12	194
Local Unemployment Rate (percent in August 1972)	137	-51 *	32	7	20	118	27	145
Non-South Region	1891 **	55	164 *	22	-29	2110 **	-8	2103 **
Distance to Nearest City[1] (0-65 miles)	-49 **	-9	-2	-0	-2	-60 **	-3	-63 **

Table 9.8 continued

	Male Earnings	Female Earnings	Assets	Welfare	Other Transfers	Taxable Income	Total Transfers	Total Income
Typical Local Male Wage (dollars per hour)	1730 **	52	162 *	6	-5	1944 **	1	1945 **
Typical Local Female Wage (dollars per hour)	2840 **	407 *	253 *	17	36	3500 **	53	3552 **
H's Sentence Completion Score (0-13)	1197 ***	113 *	45 *	-20 **	-30	1355 ***	-50 *	1305 ***
H's Age	-37 *	2	20 **	1	19 ***	-15	20 **	4
H's Education[1] (years)	882 ***	95 **	39 **	-13 **	-23 *	1015 ***	-36 **	979 ***
H's Occupational Status (Duncan Score)	139 ***	12 **	7 **	-2 **	-9 **	158 ***	-11 ***	148 ***
H's Hours > 0	9462 ***	670	-466 *	-322 **	-2849 ***	9666 ***	-3171 ***	6496 **
H's Weeks Worked[1]	200 ***	7	-8 *	-8 ***	-48 ***	199 ***	-56 ***	143 ***
H's Hrs/Wk Worked[1]	146 ***	-2	-2	-5 **	-31 ***	142 ***	-36 ***	106 **
H's Hours Worked[1]	2.77 ***	-.05	-.06	-.10 ***	-.57 ***	2.67 ***	-.67 ***	1.99 ***
H's Hourly Wage	1088 ***	-1	49 **	-6 *	-28 **	1136 ***	-34 **	1102 ***
W's Sentence Completion Score (0-13)	1463 **	103	175 *	-17	-21	1741 **	-38	1703 **
W's Age	-17	-2	20 **	·1	18 **	1	19 **	19
W's Education[1] (years)	894 ***	233 ***	51 **	-18 **	-35 **	1178 ***	-54 **	1124 ***
W's Occupational Status (Duncan Score)	6	65 ***	-0	-1	-2 *	71 ***	-3 *	68 ***
W's Hours > 0	-1418 **	3082 ***	-110	-44 *	-77	1553 **	-121	1433 **
W's Weeks Worked[1]	-34 **	86 ***	-0	-1 *	-2	52 **	-3	49 **
W's Hrs/Wk Worked[1]	-47 **	93 ***	-3	-1 *	-2	43 **	-3	40 **
W's Hours Worked[1]	-1.02 **	2.34 ***	-.02	-.02	-.03	1.29 **	-.05	1.24 **
W's Hourly Wage	134	945 ***	-2	-10	-32	1076 ***	-42 *	1034 ***

1-tailed probability of occurrence by chance

* p ≤ .005
** p ≤ .00005
*** t ≥ 10

1. A "basic variable" - Respondents with missing data on any basic variable were excluded from the multivariate analyses.
2. Husband's retrospective report, coded 1 = poor, 3 = average, 5= pretty well-off

"affects" other components of his family's income, such as his wife's earnings.

One cannot conduct an analysis of this type on all sorts of families simultaneously. For example, it is impossible to analyze the effects of Male Education in a sample that includes families with no male adult. I will therefore confine these analyses to families with both a male and female adult in the family. For this sample of families, the questions asked of the "household head" always refer to a male.

Table 9.8 shows that all measured exogenous/background characteristics except the husband's Non-School Training are strongly related to the husband's earnings, but most are only weakly (if at all) related to other sources of income. The coefficients have the predicted sign, with the possible exception of the relationship between husband's Earnings and his Father's Citizenship. Being a second generation American appears to have a positive effect on both husband's Earnings and Assets. This finding is supported in other surveys. It could be an indication of selective immigration. Non-white men earn less and receive less from Assets and Other Transfers than whites. Non-whites receive more Welfare than whites.

Children have an insignificant effect on the husband's Earnings but are related to other components of income: the greater the number of children and the younger they are, the lower the wife's Earnings, presumably because she is less likely to work. Such families may be less likely to have accumulated substantial savings or financial investments. This would explain the fact that there is a negative relationship between the Number of Children and Assets. Finally, those families with more children receive more Welfare and less Other Transfer Income. The last relationship reflects the fact that Other Transfer Income is primarily/unemployment benefits, Social Security, and private pensions.

The attitudinal variables have the expected relation to the components of income, at least in Table 9.8 where no other variables are controlled. If the husband has high educational goals for his children, high achievement motivation, or minimizes risk (according to the PSID Risk Avoidance scale), both he and his wife earn slightly more, receive more from Assets, and receive less Welfare, other things being equal.

If the male is self-employed, his Earnings and the family's Asset Income are above average, but his wife's Earnings are below average. Not surprisingly, those husbands who are handicapped or have other physical limitations earn considerably less and receive more Other Transfer Income. However, the increase in Transfer Income covers only about 20 percent of the loss in Earnings.

Living outside the South, near a city, or in communities with high average hourly wages for manual labor tends to result in higher husband's Earnings.

In discussing the bivariate relationship between the sources of income and the variables describing husbands and wives, it is convenient to divide the latter into two groups: variables which describe "economic behavior" or conditions during a specified time period (such as Hours Greater than Zero, Weeks Worked, Average Hours per Week, Total Hours Worked, and Hourly Wages) and "exogenous" variables (such as Sentence Completion Test Score, Age, Education, and Occupational Status). The exogenous variables presumably affect economic behavior, but not vice versa.

Analagous exogenous variables for husbands and wives are usually strongly correlated. Highly educated husbands tend to have highly educated wives, and so forth. Thus if husband's Education is related to husband's Earnings, wife's Education is also likely to be related to husband's Earnings. In general, this is the pattern in Table 9.8 Because four percent of the husbands are retired, the coefficients of Male and Female Age are low when predicting Earnings and high for Other Transfer Income. The other exogenous variables exhibit the predicted relationships to Earnings and Assets and small negative relationships to Transfer Income. (Female Sentence Completion Test Score is of little interest, since only eight percent of the wives had scores. The rest of the cases are assigned the mean on Female Sentence Completion Test Score.)

The most interesting relationships in Table 9.8 are between the economic behavior variables and Earnings. The very strong association between one's own economic behavior and one's own Earnings is expected.[5] The interesting aspect concerns the relationship between one's own economic behavior and the earnings of the spouse. Panel A of Table 9.9 shows the correlations among these variables. I think these correlations can best be interpreted within the framework of an elementary model of the basic work situation in which hours worked is the individual's input and earnings is his output. Wages define the relationship between the two. The individual has very little short-term control over his wages and can therefore vary his earnings only by varying his hours (including the decision to work or not work). Examination of the interspouse correlations in Table 9.9 shows that husband's Hours Worked (input) is unrelated to wife's Hours Worked or Earnings. The most significant correlation (- 0.128) is between wife's Hours Worked and husband's Earnings. This suggests that the output of husband's economic situation (Earnings) causally influences the wife's input (Hours) to her work situation. There is an insignificant correlation (- 0.020)

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5. Since the unstandardized regression coefficient of husband's Hours Worked when predicting husband's Earnings is $2.77, and since earnings equals the product of hours and wages, one might mistakenly assume that the mean of husband's Hourly Wage is $2.77 per hour. In fact

$B_{xy,x} \neq \bar{y}$, but rather

$$B_{xy,x} = \bar{y} + \frac{\mathrm{Cov}(x^2,y)}{\mathrm{Var}(x)} - \bar{x}\,\frac{\mathrm{Cov}(x,y)}{\mathrm{Var}(x)} \text{ , where}$$

$B_{xy,x}$ is the unstandardized regression coefficient of the product of x and y on x,

$\bar{y}$ is the mean of y,

$\bar{x}$ is the mean of x,

Var(x) is the variance of x,

Cov(x^2,y) is the covariance of x^2 and y, and

Cov(x,y) is the covariance of x and y.

The proof follows from the fact that $\Sigma x^2y - n\bar{x}\,\overline{xy} = \bar{y}(\Sigma x^2 - n\bar{x}^2) + (\Sigma x^2y - n\overline{x^2}\bar{y}) - \bar{x}(\Sigma xy - n\bar{x}\bar{y})$. In this instance x represents husband's Hours Worked, y represents husband's Hourly Wage, and their product xy represents husband's Earnings.

Table 9.9

A. Correlations Among Husbands' and Wives' Hours and Earnings:[a] 2245 Families with Two Adults, Complete Data on Basic Variables, and Male Non-Military, Non-Student Head Aged 25-64

	H's Hours	H's Earnings	W's Hours	W's Earnings
H's Hours	1.00000			
H's Earnings	.32188	1.00000		
W's Hours	-.02657	-.12821	1.00000	
W's Earnings	-.01602	-.02009	.82961	1.00000
Mean	2131	9224	656	1599
S.D.	773	6667	837	2357

[a] Wages have been omitted from this table because the variable is unknown for those who did not work.

B. Correlations Among Husbands' and Wives' Hours, Wages, and Earnings: 1134 Families with Two Working Adults, Complete Data on Basic Variables, and Male Non-Military, Non-Student Head Aged 25-64.

	H's Hours	H's Wage	H's Earnings	W's Hours	W's Wage	W's Earnings
H's Hours	1.00000					
H's Wage	-.20433	1.00000				
H's Earnings	.21610	.58094	1.00000			
W's Hours	.01608	-.06675	-.11794	1.00000		
W's Wage	-.04802	.17537	.27321	-.03627	1.00000	
W's Earnings	-.02326	.07974	.11803	.68877	.48840	1.00000
Mean	2174	4.253	8777	1255	2.504	3093
S.D.	619	3.724	4904	751	1.815	2493

between husband's and wife's Earnings. When we examine the same relationship for families with two working adults (Panel B) we observe a moderate positive correlation between spouses' Wages that is presumably due to the similarity in background and human capital variables. Within this subsample, wife's Wages and Earnings are positively correlated with husband's Earnings.

Multivariate Analyses

This section begins by examining a simple model for predicting the economic behavior of husbands and wives in two-adult families. In this model there are eight exogenous variables which can be separated into three groups:

1. Own Characteristics -- Age, Age^2, and Education
2. Spouse's Characteristics -- same as above
3. Other Family Characteristics -- Number of Children and Age of Youngest Child.

A major advantage of such a simple model is that it can be made symmetric, since the PSID has Age and Education for both spouses. Because PSID did not obtain other background data for wives, it is difficult to examine a more complicated symmetric model.[6]

In order to study the interplay between wages and hours worked, it is necessary to restrict the sample to those who worked. (A person's wage is not defined if he/she did not work.) Because the product of hours worked and wages equals earnings, it is conventional to treat the logs of these variables as the endogenous variables in an additive model.[7] While I will restrict the discussion to regression equations predicting Ln Wages, Ln Hours, and Ln Earnings, I examined analogous regressions for the untransformed variables and found that they support the conclusions of this section.

Table 9.10 shows three separate regressions with Ln Hourly Wages of the husband and wife as the dependent variables. Table 9.11, with one additional equation, similarly treats Ln Hours Worked. The first equation in each table regresses the dependent variable against an individual's own personal characteristics. The effect of Age on Ln Wages in Table 9.10 is similar for both husbands and wives. Ln Wages increase with age but at a decreasing rate. An extra year of education increases a husband's wage at any given age by about 7.5 percent. If the wife has an extra year of education, she typically earns about 10 percent more, assuming she works at all. (Equations not shown here indicate that this relationship is essentially linear for husbands but not for wives. Wife's wages increase at rates ranging from 4.2 percent per year of primary school to over 15 percent for each year of college.)

The second equation for each sex regresses his or her wage on all eight exogenous variables -- personal characteristics, spouse's characteristics, and other family characteristics. The coefficients of Number of Children and Age of Youngest Child are insignificant in predicting both husband's and wife's Ln Wages.

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[6] As mentioned in the previous section, for those cases in which the wife was interviewed in 1972, I have a measure of her Sentence Completion Test Score instead of her husband's. One could explore a model including the Test Score for wives and husbands (whichever is present) but the effect of Test Score would be confounded with the respondent.

[7] The multiplicative relationship between wages and hours is not perfect for females in the following analyses because of the six women who had no earnings although they had positive Hours Worked. I assigned these women a value of 0 for Ln Wages.

Table 9.10

Unstandardized Regressions of 1971 Male and Female Ln Wages on Characteristics of 1134 Households with Male Non-military, Non-student Head, Spouse Present, Complete Data on All Basic Variables, and Both Spouses Working at Some Time.

		Male Ln Wages			Female Ln Wages		
		Eq.1	Eq.2	Eq.3	Eq.1	Eq.2	Eq.3
Own Education	B	.07485	.06393	.06009	.10032	.07720	.07346
	s.e.	(.00482)	(.00582)	(.00580)	(.00680)	(.00823)	(.00817)
Own Age	B	.06767	[.00189]	[.00712]	.03834	.07236	.06007
	s.e.	(.01130)	(.01923)	(.01904)	(.01200)	(.02021)	(.02012)
Own Age^2	B	-.00072	[-.00006]	[-.00011]	-.00039	-.00081	-.00068
	s.e.	(.00013)	(.00021)	(.00021)	(.00015)	(.00025)	(.00024)
Number of Children	B		[.00976]	[.01138]		[-.01210]	[-.01380]
	s.e.		(.01441)	(.01425)		(.01645)	(.01626)
Age of Youngest Child	B		[.00274]	[.00183]		[.00678]	[.00630]
	s.e.		(.00409)	(.00405)		(.00467)	(.00462)
Spouse's Education	B		.02146	[.01111]	+	.02862	.01745
	s.e.		(.00721)	(.00741)		(.00664)	(.00691)
Spouse's Age	B		.07037	.06067		[-.03903]	(-.03936)
	s.e.		(.01770)	(.01760)		(.02195)	(.02170)
Spouse's Age^2	B		-.00077	-.00066		[.00042]	[.00043]
	s.e.		(.00021)	(.00021)		(.00024)	(.00024)
Spouse's Ln Wages	B	+	+	.13413	+	+	.17472
	s.e.			(.02583)			(.03364)
R^2		.19092	.21086	.22936	.16901	.19155	.21050
S.D. of Residuals		.51447	.50922	.50344	.58793	.58119	.57460
Constant		-1.09329	-1.23625	-1.07000	-1.33867	-1.23944	-1.02344
Other Variables *							
Non-South Origin					+		
Non-Farm Origin		+	+	+	+	+	+
City Origin		+	+	+	+	+	
Male Sentence Completion Test Score		+	+	+			
Siblings (of Head)					-		
Risk Aversion		+	+	+	+	+	
Low Educational Goals for Children					-		
Distance to City		-	-	-	-	-	
Non-South Region		+	+	+	+	+	+
Unemployment Rate		+	+	+			
Typical Male Wage		+	+	+			
Typical Female Wage		+	+	+	+		
Union Member		+	+	+			
Male Labor Shortage		-	-	-			
Self-Employed (Male)		-	-	-	-	-	
Job Tenure		+	+	+			
Occupation		+	+	+	+	+	+
Spouse's Occupation		+					
Spouse's Ln Earnings		+			+	+	
Spouse's Education^2				-	+		
Own Education ^{2}					+	+	+

*The sign corresponds to the sign of the regression coefficient if that variable (alone) were entered as the next step of the equation. A sign appears only if the addition of the variable to the equation would result in a coefficient with an F ≥ 10.

This should not be surprising. We expect these variables to affect a wife's decision on how much to work (if at all), but not to affect how much she is paid if she works. The coefficients of husband's Age and Age^2 are also insignificant in predicting wife's Ln Wages. Husband's Education is significiant in the wife's Ln Wage equation but this turns out to be because it is picking up some of the non-linear effect of wife's Education. When I added wife's Education^2 to the equation (not shown here), the coefficient of husband's Education approached zero. More surprising, the coefficients of all the wife's characteristics are significant in the husband's Ln Wage equation. Wife's Age and Age^2 prove to be more strongly related to husband's Ln Wages than the husband's own age variables. The wife's education also seems to have a direct "effect" on her husband's wages. Among men at a given educational level, well-paid men are more likely than poorly paid men to be married to well-educated women. Since it is hard to believe that many employers are directly interested in a spouse's traits, these results may suggest that men with high potential earnings are more likely to marry well-educated women, even with their education controlled. (In equations predicting husband's untransformed wages the effects of wife's characteristics are much smaller.)

Equation 3 of Table 9.10 adds spouse's Ln Wages to the variables in Equation 2. Because I am treating the dependent variable of one equation as an independent variable in a second equation (holding all the other exogenous variables constant), the partial correlation coefficient between spouses' Ln Wages in Equation 3 is an alternative measure of their association. Controlling the eight exogenous variables, this partial correlation between spouses' Ln Wages is 0.153 and very significant. It seems unlikely that spouse's Ln Wages have a major direct effect on own Wages. I therefore assume that spouses must resemble each other with respect to other traits not included in Equation 3 that affect their wages. These excluded variables include IQ, race, community characteristics, and so forth. Controlling the available community characteristics plus race reduces the partial correlation to 0.115. Selective mating on other traits presumably accounts for part of the remaining partial correlation.

Unfortunately, we cannot be certain that this generalizations holds for households in general, as against households where both spouses work. Conventional economic theory suggests that an increase in the husband's earning power relative to the wife will lead to a decline in the wife's desire to work. Thus even if there were no partial correlation between husbands' and wives' potential wages for the population as a whole, one would expect to find a modest positive correlation among couples who both work.

The bottom of Table 9.10 lists all the other measured variables from Table 9.8 that were very significantly correlated with spouse's Ln Wages after controlling the variables in the top of the table. It shows all variables that would enter the equation with an F-level greater than 10.

Table 9.11 shows the equations predicting Ln Hours Worked. Own Education has a small positive relationship with annual hours for men and women. For men, Age has a significant curvilinear relationship to Ln Hours Worked. One percent of all men are in semi-retirement or retired during the year while other older men may have reduced the number of hours they work. The unstandardized coefficients of the wife's age variables in her Ln Hours equation are similar to those of the husband's age in Equation 1. **But the** standard errors for females are larger than for males because female hours are more variable, so **the age coefficients for wives are insignificant**.

When variables describing other family members are added in Equation 2, the only additional variable related to husband's Ln Hours is the Age of the Youngest Child. Men in families with young children work more hours. One is tempted to argue that husbands compensate for the wife's tendency to work less or not at all when there are young children. But inclusion of spouse's Ln Hours Equation 4 raises the magnitude of the coefficient of Age of Youngest Child. Perhaps men with young children feel a greater need for earnings (or for a sense of financial security) for their families. Accordingly, they might work longer hours or take a second job, since hours are **more subject to** individual control than wages.

The second equation for wife's Ln Hours is more interesting. The negative effect of children, especially young children, on wife's Ln Hours is clearly reflected in the regression coefficients. In addition, wife's Age exhibits a stronger curvilinear relationship to her Ln Hours. Finally, the coefficients of wife's and husband's Education have opposite signs. Equations 2, 3, and 4 for wife's Ln Hours all indicate that the more education her husband has, the less hours she will work. This could be because highly educated men earn more. But when husband's Ln Earnings is added

Table 9.11

Unstandardized Regressions of 1971 Male and Female Ln Hours on Characteristics of 1134 Households with Male Non-military, Non-student Head, Spouse Present, Complete Data on All Basic Variables, and Both Spouses Working at Some Time.

		Male Ln Hours				Female Ln Hours			
		Eq.1	Eq.2	Eq.3	Eq.4	Eq.1	Eq.2	Eq.3	Eq.4
Own Education	B	.00876	[.00463]	.01278	.01427	[.00774]	.03047	[.02164]	[.01897]
	s.e.	(.00402)	(.00488)	(.00508)	(.00509)	(.01222)	(.01458)	(.01512)	(.01508)
Own Age	B	.04241	.04489	.04513	.04743	[.03855]	.10469	.09641	.09415
	s.e.	(.00942)	(.01615)	(.01597)	(.01593)	(.02157)	(.03579)	(.03593)	(.03580)
Own Age^2	B	-.00057	-.00059	-.00060	-.00063	[-.00032]	-.00142	-.00132	-.00130
	s.e.	(.00011)	(.00018)	(.00018)	(.00018)	(.00027)	(.00043)	(.00044)	(.00043)
Number of Children	B		[-.01966]	[-.01841]	[-.01544]	-	-.08224	-.08085	-.07634
	s.e.		(.01210)	(.01197)	(.01197)		(.02913)	(.02909)	(.02901)
Age of Youngest Child	B		-.00983	-.00948	-.01037	+	.02432	.02355	.02572
	s.e.		(.00343)	(.00340)	(.00340)		(.00827)	(.00826)	(.00826)
Spouse's Education	B		[.00965]	.01238	[.01124]	-	-.04426	-.04754	-.04877
	s.e.		(.00606)	(.00601)	(.00600)		(.01176)	(.01184)	(.01179)
Spouse's Age	B		[.00794]	[.01691]	[.01298]		[-.06334]	[-.05888]	[-.06876]
	s.e.		(.01487)	(.01480)	(.01481)		(.03887)	(.03886)	(.03884)
Spouse's Age^2	B		[-.00007]	[-.00016]	[-.00011]		[.00083]	[.00078]	.00091
	s.e.		(.00018)	(.00018)	(.00018)		(.00043)	(.00043)	(.00043)
Own Ln Wages	B	-	-	-.12749	-.12569			.11435	.12081
	s.e.			(.02476)	(.02468)			(.05272)	(.05255)
Spouse's Ln Hours	B				.03633				.22573
	s.e.				(.01221)				(.07143)
R^2		.05369	.06318	.08478	.09194	.01851	.07294	.07680	.08494
Constant		6.80255	6.61474	6.45714	6.23631	5.71685	6.13935	6.28108	4.79592
S.D. of Residuals		.42879	.42758	.42281	.42134	1.03679	1.02935	1.02766	1.02358
Other Variables*									
Race		+	+	+	+	-	-	-	-
Non-South Origin						-			
Physical Handicap		-	-	-	-				
Shortage of Male Labor						-	-	-	-
Shortage of Female Labor		-	-	-					
Self-Employed (male)		+	+	+	+				
Job Tenure (male)		+	+	+	+				
Own Occupation		+	+	+	+				
Male Sentence Completion Test Score				+	+				
Risk Aversion				+	+				

*The sign corresponds to the sign of the regression coefficient if that variable (alone) were entered as the next step of the equation.

to the equation, its coefficient is insignificant but husband's Education retains its effect. In a sample that included all households, this finding would suggest that men with more education had prejudices against their wives' working, or that women who did not want to go to work tended to marry highly educated men. However, neither of these explanations is very plausible in an analysis restricted to women who did work. I have no explanation for this observed relationship.

Equations 3 and 4 of Table 9.11 add own Ln Wages and spouse's Ln Hours to the equations predicting Ln Hours Worked. (Spouse's Ln Wages were not significantly related to own Ln Hours after controlling own Ln Wages.) The coefficients of the eight exogenous variables remain very similar to those in the second equation, except that own Education becomes more important for men and less important for women after controlling own Ln Wages. Surprisingly, there is a positive relationship between spouse's and own Ln Hours Worked for both husbands and wives, even after controlling the eight exogenous variables and own Ln Wages. This directly contradicts conventional wisdom, which would predict a pattern of substitution between spouses' hours. It is possible that women (especially those without young children) whose husbands work long hours will choose to work longer hours rather than spend time alone at home. Note, however, that the sample is restricted to spouses who both worked some hours. As noted later, the relationship disappears when one includes non-workers as well as workers in the sample.

Husband's Ln Wages are negatively related to his Ln Hours, while wife's Ln Wages are positively related to her Ln Hours. Classical utility theory could explain both results as a function of the difference in average wage rates for men and women (4.253 and 2.504 dollars per hour respectively). For any given individual, there must be a series of different combinations of income and leisure that all seem equally desirable. These points define an "indifference curve" that represents a specific level of well-being ("utility"). Every point on a particular indifference curve represents a combination of income and leisure as desirable as any other combination represented by the other points on the curve. Figure 1 shows several such curves. The are more desirable than those on curve (1), and so on. But for any given individual, the trade-off between income and leisure is a straight line, not a curve. The slope of this line depends on the individual's hourly wage rate, since for each hour less of leisure (i.e. each hour of work) his or her income increases by an amount equal to his or her hourly wage. These lines are called wage constraints because they represent the possible combinations of leisure and income that are available to an individual with a given wage. All wage constraint lines must intersect at the leisure axis, because everyone has the same fixed amount of total time available. Figure 1 shows two "low wage" lines, labelled A and B, and two "high wage" lines, C and D. Utility theory predicts that each individual will choose the combination of leisure and income where his wage constraint line intersects the highest utility curve. These points are indicated in Figure 1 as a_1, b_2, c_3, and d_4. The arrows between them show how income and hours of leisure would change if an individual moved from wage A to wage B or from wage C to wage D. Those with low wages (in this case women) will respond to an increase in wages by working more (consuming less leisure). For them, leisure is an "inferior good" because they choose less of it when their wage constraint increases. Men, in comparison to women, work more hours and receive higher wages, as indicated in the figure. For them, an increase in wages is associated with both an increase in income and a decrease in hours worked (increase in leisure). This is the typical pattern of substitution among "superior" commodities that is discussed in the ec[illegible] literature. (See Becker, 1965.)[8]

An alternative explanation of the positive relationship between women's hours and wages is that many women have part-time jobs which tend to pay less and obviously involve fewer hours. This reasoning does not apply to men because most of them work full-time. The negative relationship between their hours and wages could be due to a tendency for better-paying jobs to be associated with shorter work weeks. Also, men who work overtime and/or who have a second job

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8. The indifference curves in Fugure 1 are hypothetical. They have been drawn to demonstrate the logical possibility of men and women reacting differently to an increase in wages. One could draw an alternative figure which would imply similar reactions for men and women.

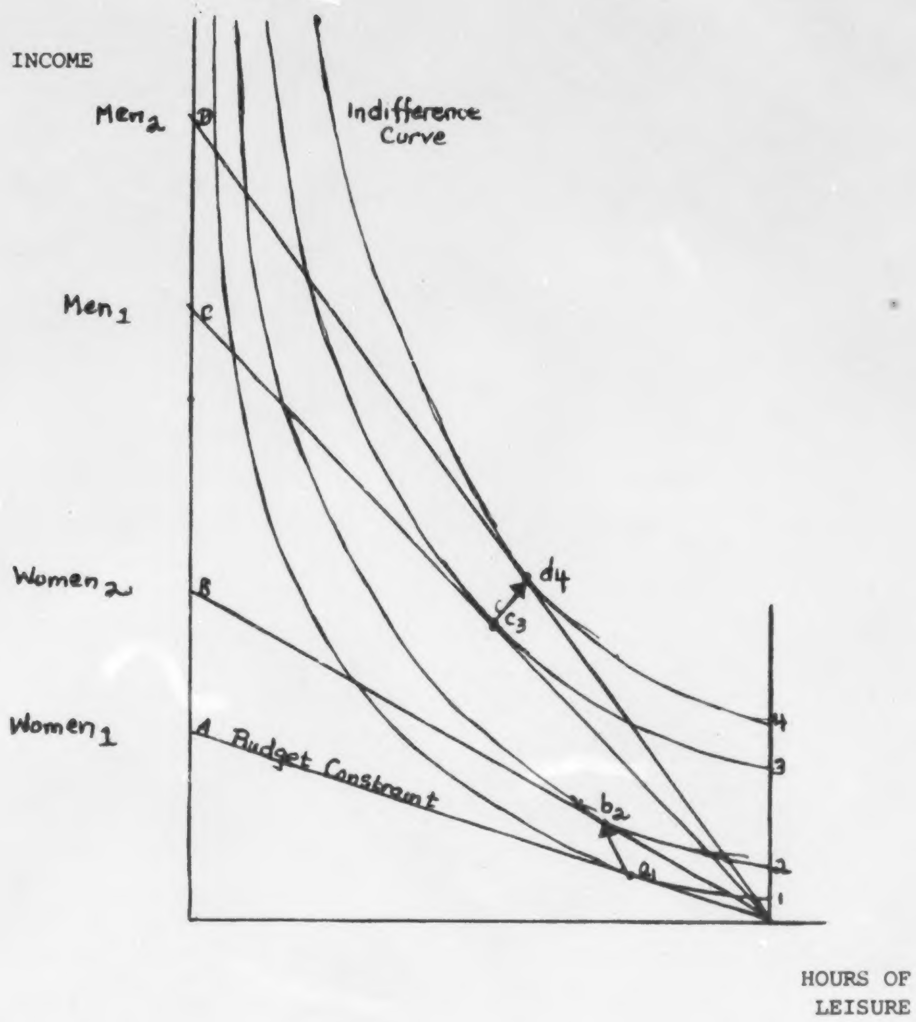


Figure 1 - Hypothetical Relationship between Hours, Wages and Utility for Men and Women.

are likely to do so because they are paid low wages and need the money. If this view that part-time workers and second-job holders receive lower wages than other full-time workers were true, one would expect to observe a curvilinear relationship between hours worked and wages received. However, in the PSID data there is no such relationship between Female Hours and Wages. Male Wages and Hours exhibit a significant negative relationship with no significant deviation from linearity.

Some of the variables which were not entered into the equations exhibit interesting patterns. First, Race has opposite effects on husband's and wife's Hours. An obvious explanation is that Black men face a much higher unemployment rate than Whites, thus decreasing their hours. Because of this and the fact that employed Black men earn much lower wages, there is more pressure on Black wives to work. Controlling husband's hours and wages does not, however, appreciably reduce the effect of race on the wife's hours. Thus if black male unemployment really explains black female hours, it does so by making black wives reluctant to depend on their husbands' earning power even when his earnings are relatively high. The other interesting relationship is that self-employed husbands work more hours and earn lower wages (see Table 9.10). As a result, their total earnings do not differ significantly from those of salaried men of similar age and education.

The results from Tables 9.10 and 9.11 can be summarized as follows:

1. Own Age has a curvilinear effect on both Wages and Hours. Both curves peak in middle age.
2. Wife's Age is, perversely, a better predictor of husband's Ln Wage than his own Age.
3. Education affects Earnings predominantly through its influence on wages, not hours.
4. The Age of Youngest Child and Number of Children have major effects on wife's Hours and slight effects on husband's Hours, but they are unrelated to either husband's or wife's Wages.
5. Spouses' Wages are positively related. So are spouses' Hours.

The above multivariate approach to relating wages and hours of husbands [illegible] is primarily suited to the analysis of families with two working adults.

However, this represents only about half of all two-adult families and slightly more than a third of all families. I therefore computed comparable regressions (not presented here) predicting Ln Hours and Ln Earnings for the sample of all two-adult families. (I did not try to predict Wages since it is not possible to determine what wage a non-worker would command if he or she worked.) Since the log of zero is undefined, all zeros were recoded to 1. As was seen earlier (see Table 9.4), including non-workers in the analysis greatly increases the standard deviations of Ln Hours and Ln Earnings for both husbands and wives. The variance in these two variables is primarily associated with the dichotomy between those who did and did not work. In the sample of two-adult families with complete data, 51 percent of the wives and 96 percent of the husbands worked during at least part of 1971.

With two exceptions, the regressions predicting Ln Hours for this larger sample were similar to those in Table 9.11. The first exception is that the Number of Children has a significant negative effect on husband's as well as wife's Hours. Second, and more interesting from an economic perspective, there is no linear relationship between spouses' Ln Hours after controlling the age, education and children variables in Table 9.11. In particular, these additional regressions exhibit neither the positive association between spouses' hours evidenced in the sample of families with two working adults, nor the pattern of substitution predicted by conventional economics. **It seems that the wives of husbands who work long hours tend to either not work at all or work long hours themselves.**

The regressions predicting Ln Earnings conform more closely to traditional economic theory. For both husbands and wives, after controlling the eight exogenous variables and own Ln Hours, spouse's Ln Hours has a significant negative coefficient while spouse's Ln Earnings has a positive coefficient. This implies that among men or women who work a given number of hours, those who have spouses who work many hours also tend to have low earnings although the causality presumably operates from own earnings to spouse's hours. This corresponds to the "substitution" effect. The positive relationship between spouses' earnings after controlling hours worked is due to the positive association between working spouses' wages discussed previously. As I suggested earlier, this relationship is probably due to selective mating and the common effect of local economic conditions on the wages of both spouses. The conclusion that spouses' wages are not causally related receives additional support later in the examination of the relationship between changes in spouses' earnings. The regressions predicting Ln Earnings are also similar to those predicting wages (Table 9.10) in that once again the wife's age variables are inexplicably more important than the husband's in predicting his earnings.

Analysis of Four-Year Income for Stable Families

The preceding discussion has focused on the income received by a sample of families during 1971. Because of more or less random fluctuations and measurement error, an analysis of a single year is not completely satisfactory. A large proportion of this randomness can be controlled by averaging several independent measures of economic behavior or success over four or five successive years. I restricted this analysis to the sample of stable families/(defined on page 2) with complete income data over a four-year period. Because I think that there is a relationship between economic stability and family stability, I expected the sample of stable families to display less variation in the cross-sectional economic data than the sample of all families. This is indeed the case.

Table 9.12 presents some comparisons between the cross-sectional data for the full 1972 sample and the sub-sample of stable families, as well as the four-year-average data for the stable families. I did not include in the averages the 1967 economic data (obtained in the 1968 interview) because some variables were not available for 1967 and because the first year of interviewing did not obtain

Table 9.12

Means and Standard Deviations for 3160 Households with Complete Data on Basic Variables and with Non-student, Non-military Head Aged 25-64, and for Subsample of 2005 Households with No Change in Head or Spouse from 1968 to 1972, Complete Data on Economic Variables from 1968 to 1971, Head Not a Student from 1968 thru 1971 or in Military in 1971. All Income Variables in 1967 dollars.

	Means			Standard Deviations		
	All Families	Continuous Families		All Families	Continuous Families	
	1971	1971	1968-71 Average	1971	1971	1968-71 Average
Male Adult Variables for families with a male adult	N=2500	N=1654	N=1654	N=2500	N=1654	N=1654
Weeks Worked	44.339	44.583	45.775	12.275	12.031	8.879
Earnings	8942.990	9013.147	8927.021	6582.306	6171.201	5257.577
Earnings$^{1/3}$	19.257	19.373	19.864	6.034	5.988	4.472
Ln Earnings	8.537	8.558	8.818	1.968	1.977	1.248
Proportion with Positive Earnings	.957	.956	.986			
Female Adult Variables for families with a female adult	N=2907	N=1916	N=1916	N=2907	N=1916	N=1916
Weeks Worked	21.784	20.960	20.945	22.047	22.140	20.042
Earnings	1963.748	1935.617	1868.630	2659.481	2675.468	2428.524
Earnings$^{1/3}$	7.878	7.621	8.402	7.634	7.718	7.025
Ln Earnings	4.378	4.204	4.925	3.901	3.940	3.572
Proportion with Positive Earnings	.572	.545	.691			
Other Income Variables	N=3160	N=2005	N=2005	N=3160	N=2005	N=2005
Asset Income	362.369	333.400	313.171	1195.396	936.086	858.930
Taxable Income	9233.614	9618.922	9463.626	7353.197	6908.707	6159.501
Taxable Income$^{1/3}$	19.000	19.503	19.756	6.918	6.651	5.819
Ln Taxable Income	8.391	8.516	8.666	2.215	2.117	1.748
Proportion with Positive Taxable Income	.946	.952	.973			
Welfare	105.626	104.994	100.458	516.069	526.423	500.837
Other Transfer	410.526	363.276	298.941	1046.150	983.709	823.143
Total Transfer	516.152	468.270	399.398	1162.292	1108.907	921.970
Family Income	9748.766	10087.192	9863.024	7046.671	6582.868	5891.593
Family Income$^{1/3}$	20.229	20.620	20.623	4.986	4.726	4.210
Ln Family Income	8.915	8.992	9.011	.897	.812	.663
Proportion with Positive Family Income	.998	.998	1.000			

Table 9.12 continued

Comparison of Samples on Basic Correlations

		All Families	Continuous Families	
		1971	1971	1968-71 Average
Male Education with:	(Restricted to			
Male Earnings	families with a	.408	.460	.487
Male Earnings$^{1/3}$	male adult)	.395	.430	.483
Male Ln Earnings		.272	.295	.325
Female Education with:	(Restricted to			
Female Earnings	families with	.301	.326	.318
Female Earnings$^{1/3}$	a female adult)	.233	.251	.247
Female Ln Earnings		.186	.205	.193
Race with:				
Family Income		.186	.210	.245
Family Income$^{1/3}$		.228	.244	.297
Ln Family Income		.208	.229	.311
Father's Education with:				
Family Income		.213	.251	.251
Family Income$^{1/3}$		.223	.246	.257
Ln Family Income		.194	.210	.245
Father's Occupation with:				
Family Income		.177	.191	.211
Family Income$^{1/3}$		.188	.190	.208
Ln Family Income		.167	.166	.192

as good data as later years. For the 1971 variables the stable families tend to have a higher mean than the full sample. The standard deviations of the 1971 variables are usually 3 to 10 percent lower for the stable families than for the complete sample. Within the stable sample, the standard deviations of four-year averages are 10 to 20 percent lower than the standard deviations for a single year. The correlations of background and human capital variables with four-year averages are also slightly larger than the correlations with the 1971 economic variables. A few examples appear in Table 9.12.

Based on these comparisons between the cross-sectional and longitudinal data one would expect to obtain the best-fitting multivariate model of family income by predicting its four-year average with the sample of stable families. These expectations are confirmed. Furthermore, models predicting either Family $\text{Income}^{1/3}$ or Ln Family Income fit much better than those predicting Family Income. For this reason, several equations predicting Ln Family Income are presented in Table 9.13. (There were no families with non-positive four-year average income. This eliminated the usual problems with taking logarithms in cross-sectional data.) At the bottom of the table I present the R^2 and standard deviation of the residuals from regressing other dependent variables on the same independent variables. Despite the fact that we can explain a higher proportion of the variance of Taxable Income than Family Income (as measured by R^2), the margin of error (as measured by the standard deviation of the residuals) in predicting Taxable Income is even larger than in predicting Family Income. This paradoxical situation can occur because Taxable Income has a greater variance than Family Income.

Two specific features of Table 9.13 are noteworthy. First, in contrast to the analyses discussed earlier, the male age variables predominate over the female age variables. Second, Female Education dominates the effect of the

Table 9.13

Unstandardized Regressions of Ln Mean 1968-1971 Family Income (1967 dollars) for 2005 Households With No Change in Head or Spouse Between 1968 and 1972 Interviews, Complete Data on All Basic Variables, And on 1968-1971 Economic Variables With Non-military, Non-student Head Aged 25-64.

		Eq.1	Eq.2	Eq.3	Eq.4	Eq.5
Family's Adult Structure						
Single Female Present	B	-.92023	-.87196	.86986	.71269	.62240
	s.e	(.03290)	(.03024)	(.31211)	(.30194)	(.29787)
Single Male Present	B	-.63790	-.57424	.49229	[.19695]	[.19739]
	s.e.	(.06072)	(.05464)	(.23866)	(.23191)	(.22833)
Background Variables						
White	B	+	.27572	[-.04466]	[.00423]	[.04560]
	s.e.		(.03514)	(.07655)	(.07366)	(.07273)
Father's Education	B	+	.03166	.00941	.00895	.00816
	s.e.		(.00458)	(.00412)	(.00391)	(.00385)
Missing Data on Father's Education	B	-	-.23037	[-.09367]	[-.04858]	[-.08107]
	s.e.		(.06379)	(.05575)	(.05261)	(.05216)
Father's Occupation	B	+	-.00324	[-.00208]	[-.00150]	[-.00167]
	s.e.		(.00148)	(.00129)	(.00122)	(.00120)
Missing Data on Father's Occupation	B	-	-.09383	[-.06471]	[-.04743]	[-.03182]
	s.e.		(.04285)	(.03731)	(.03522)	(.03474)
Father White Collar	B	+	.19833	[.06920]	[.06282]	[.06855]
	s.e.		(.05921)	(.05167)	(.04893)	(.04819)
Parent's Economic Situation	B	+	[.01131]	[-.00016]	[-.00488]	[-.00385]
	s.e.		(.00872)	(.00768)	(.00727)	(.00716)
Father U.S. Citizen	B	-	[-.01355]	[-.02809]	[.00027]	[.01548]
	s.e.		(.03260)	(.02880)	(.02729)	(.02701)
Number of Siblings	B	-	-.02117	[-.00130]	[.00138]	[.00079]
	s.e.		(.00485)	(.00430)	(.00408)	(.00401)
Non-South Origins	B	+	.12054	[.03382]	[.04005]	[.01168]
	s.e.		(.02700)	(.02373)	(.02246)	(.03070)
Non-Farm Origins	B	+	.13543	.10652	.10300	.07558
	s.e.		(.02964)	(.02583)	(.02439)	(.02426)
City Origins	B	+	.12391	.08189	.07088	[.04448]
	s.e.		(.02775)	(.02411)	(.02277)	(.02279)
Age of Male Adult	B	-	[-]	.04809	.04754	.04786
	s.e.			(.01234)	(.01189)	(.01170)
Age^2 of Male Adult	B	-	[-]	-.00050	-.00050	-.00051
	s.e.			(.00013)	(.00013)	(.00012)
Age of Female Adult	B	[-]	[-]	.02168	[.01515]	[.01494]
	s.e.			(.01038)	(.01009)	(.00993)
Age^2 of Female Adult	B	-		-.00026	[-.00021]	[-.00020]
	s.e.			(.00011)	(.00011)	(.00011)

		Eq.1	Eq.2	Eq.3	Eq.4	Eq.5
Male Sentence Completion Score	B	+	+	.03675	.02917	.03027
	s.e.			(.00646)	(.00613)	(.00603)
Female Sentence Completion Score	B	+	+	.02399	[.00770]	[.00860]
	s.e.			(.00996)	(.00947)	(.00933)
Education Variables						
Male Education	B	+	+	[.00723]	[-.00084]	[-.01083]
	s.e.			(.01546)	(.01482)	(.01472)
Male Education^2	B	+	+	[.00087]	[.00106]	[.00133]
	s.e.			(.00070)	(.00067)	(.00067)
Female Education	B	+	+	.05620	.04398	.04319
	s.e.			(.00473)	(.00458)	(.00452)
White x Head's Education	B	+	+	.01408	[.00805]	[.00856]
	s.e.			(.00676)	(.00651)	(.00642)
Black x Female Head	B	-	-	-.25310	-.19197	-.17008
	s.e.			(.06680)	(.06575)	(.06498)
Non-School Training	B	+	[+]	+	.05800	.04901
	s.e.				(.02220)	(.02190)
Physical Handicap	B	-	-	-	-.23250	-.23507
	s.e.				(.02613)	(.02581)
Family Characteristics						
Number of Children	B	-	[-]	-	[.01789]	[.01438]
	s.e.				(.01779)	(.01753)
Age of Youngest Child	B	+	+	+	.00701	.00633
	s.e.				(.00290)	(.00286)
Number of Children x 2-adult Family	B	-	[-]	-	[-.00816]	[-.00623]
	s.e.				(.01700)	(.01675)
Age Married	B	+	[+]	-	[-.00396]	[-.00311]
	s.e.				(.00219)	(.00216)
Missing Data on Age Married (or not)	B	+	+	+	[.01004]	[.00967]
	s.e.				(.04892)	(.04817)
Attitudinal Variables						
Low Educational Goals for Children	B	-	-	-	[-.00483]	[-.00371]
	s.e.				(.00990)	(.00975)

		Eq.1	Eq.2	Eq.3	Eq.4	Eq.5
Achievement Motivation	B	+	+	+	.00773	[.00628]
	s.e.				(.00366)	(.00361)
Risk Aversion	B	+	+	+	.07419	.07512
	s.e.				(.00707)	(.00696)
Community Variables						
County Unemployment Rate	B	[+]	–	[–]	[–]	[–.00416]
	s.e.					(.00451)
Distance to Nearest City	B	–	–	–	–	–.00343
	s.e.					(.00045)
Non-South Region	B	+	[+]	[+]	+	[.03401]
	s.e.					(.03166)
R^2		.29624	.44052	.58440	.63390	.64586
Constant		9.19972	8.60037	6.00724	6.19720	6.34899
S.D. of Residuals		.55664	.49780	.43024	.40483	.39846

R^2 and S.D. of Residuals
for Analogous Equations
for 5 Year Averages of Other Income
Variables

		Eq.1	Eq.2	Eq.3	Eq.4	Eq.5
Family Income	R^2	.177	.308	.487	.514	.522
	S.D. of Res.	5348	4920	4243	4141	4111
Family Income$^{1/3}$	R^2	.270	.418	.580	.622	.633
	S.D. of Res.	3.599	3.222	2.744	2.608	2.571
Taxable Income	R^2	.201	.328	.501	.534	.541
	S.D. of Res.	5509	5068	4376	4239	4213
Taxable Income$^{1/3}$	R^2	.304	.425	.556	.650	.656
	S.D. of Res.	5.041	4.594	4.045	3.606	3.578

linear and quadratic male education variables. When I remove the quadratic male education variable (in equations not shown here) the coefficient of the linear term becomes statistically significant but is consistently less than half the coefficient for Female Education

I have no explanation for these two results except to suggest that there is much multi-collinearity among some of the variables. Finally, the large changes in the coefficients of Single Female Present and Single Male Present are due to the assignment of zeros to the missing persons.

The age and education of a "non-existent"male are zero, for example.

The availability of longitudinal economic data allows one to investigate the annual fluctuations that are normally treated as random variation. Much of this variation is probably associated with national and local fluctuations in economic conditions that are beyond the scope of this chapter. However, fluctuations in one component of family income may affect others. In particular, I hypothesized that for married couples, changes in one spouse's earnings would be negatively related to changes in the other spouse's earnings. If one thinks of the family as a production unit wishing to attain a specific level of income, then one expects that when income from one source declines, family members will try to increase the unit's income from other sources, by adjusting hours worked. To test this hypothesis, I regressed the change in own Earnings on the change in spouse's Earnings, controlling own and spouse's Earnings last year. I examined such regressions (not shown here) for all possible years and for several transformations of earnings and found no relationship approaching significance. Since there is not even a consistent sign pattern, the predicted negative relationship receives no support from the PSID data. To allow for possible delays in the predicted effect, I also tested this hypothesis using two-year rather than one-year intervals. Again there was no significant relationship. This raises serious questions about standard economic interpretations of the cross-sectional relationships between husbands' and wives' hours and earnings, discussed earlier.

Conclusions

In this chapter I have used a wide range of analytical tools to explore several aspects of family income. I conclude with a summary of the main findings.

Male Earnings and Female Earnings are the two largest components of Family Income, comprising 91 to 95 percent of the total for families with heads aged 25-64. The relationship between these two components is ambiguous; their correlation is positive if the sample is restricted to families with two working adults but negative if all families are included. If the sample is restricted to working couples, there is a positive correlation between spouses' hours as well as wages. This contradicts the "substitution" theory of economic behavior among married couples. In addition, an examination of the longitudinal data on all stable families shows that changes in the earnings of one spouse are unrelated to changes in the earnings of the other.

I have shown that a family's composition is related to the sources from which a family receives income as well as the total amount of family income. As one might expect, a family with a stable composition tends to have a somewhat higher income which is also more stable. Perhaps this reflects the ideal of the "middle American" family which has a reliable (low risk) income, primarily from earnings in steady employment.

I have also shown that transfer income has a strong negative relationship to taxable income, and that the existing distribution of transfers reduces economic inequality. However, alternative distribution schemes could reduce inequality more dramatically at less cost to most taxpayers.

Appendix: Table 9.14

Means and Standard Deviations of All Variables Referred to in This Chapter for Four Subsamples With Complete Data on All Basic Variables and Non-military, Non-student Head Aged 25-64

	Mean				Standard Deviations			
	1 All Families N=3160	2 Families With 2 Adults N=2245	3 Families With 2 Working Adults N=1134	4 All Continuous Families N=2005	1 All Families N=3160	2 Families With 2 Adults N=2245	3 Families With 2 Working Adults N=1134	4 All Continuous Families N=2005
Background Variables								
Race (White)	.857	.892	.883	.859	.350	.311	.322	.348
Father's Education	8.561	8.643	8.714	8.477	2.986	3.053	3.074	2.847
Father's Occupation	27.790	27.724	28.282	26.984	17.278	17.283	17.557	16.672
Father White Collar	.230	.222	.236	.208	.402	.399	.409	.389
Parents' Economic Situation	2.305	2.294	2.244	2.244	1.410	1.398	1.403	1.385
Father U.S. Citizen	.841	.839	.850	.840	.363	.365	.354	.364
Number of Siblings	3.771	3.724	3.672	3.834	2.503	2.484	2.451	2.494
Non-Farm Origin	.694	.685	.688	.686	.461	.465	.464	.464
Non-South Origin	.668	.682	.672	.675	.471	.466	.470	.469
City Origin	.307	.291	.280	.297	.461	.454	.449	.457
1971 Male Adult Variables for families with a male								
Age	42.889	42.962	41.793	44.132	11.220	11.029	10.929	10.855
Sentence Completion Score	9.852	9.907	9.972	9.857	1.917	1.835	1.740	1.909
Score missing	.091	.100	.098	.088	.287	.300	.297	.284
Education	11.681	11.714	11.964	11.535	3.822	3.413	3.230	3.393
Occupation	38.597	39.039	40.463	38.399	21.920	21.728	21.438	21.698
Hours Worked > 0	.957	.951	1.000		.203	.193	0.000	
Hours Worked	2104.673	2131.017	2174.336		799.195	773.495	618.882	
Ln Hours Worked	7.293	7.311	7.625		1.624	1.540	.440	
Weeks Worked	44.339	44.714	46.378	44.583	12.275	11.774	7.514	12.031
Hours/Week Worked	42.870	43.200	44.057		13.720	13.277	8.996	
Wages	4.273	4.399	4.253	4.367	4.209	4.355	3.724	4.592
Ln Wages	1.247	1.281	1.286	1.272	.661	.645	.571	.647
Earnings	8942.990	9224.378	8776.546	9013.147	6582.306	6666.762	4903.738	6171.201
Earnings$^{1/3}$	19.257	19.570	19.936	19.373	6.034	5.860	3.820	5.988
Ln Earnings	8.537	8.619	8.910	8.558	1.968	1.872	.691	1.977
1971 Female Adult Variables for families with a female								
Age	41.408	39.988	38.563	42.252	11.671	11.260	10.732	11.296
Sentence Completion Score	9.718	9.878	9.864	9.771	1.253	.623	.628	1.157
Score missing	.701	.905	.906	.749	.458	.294	.292	.434
Education	11.548	11.702	12.096	11.536	2.800	2.648	2.592	2.769
Occupation	21.810	19.806	38.506	21.097	25.125	24.627	21.592	25.114
Hours Worked > 0	.572	.519	1.000		.494	.500	0.000	
Hours Worked	788.709	656.448	1255.112		894.690	837.036	751.488	
Ln Hours Worked	3.958	3.524	6.785		3.511	3.480	1.065	
Weeks Worked	21.784	18.855	36.233	20.960	22.047	21.457	15.892	22.140
Hours/Week Worked	19.733	17.341	33.282		19.398	18.915	12.152	
Wages	1.457	1.269	2.504	1.419	1.999	1.796	1.815	1.984
Ln Wages	.415	.374	.729	.419	.618	.590	.644	.602
Earnings	1963.748	1598.894	3093.456	1935.617	2659.481	2357.356	2493.109	2675.468
Earnings$^{1/3}$	7.878	6.835	13.183	7.621	7.634	7.368	4.611	7.718
Ln Earnings	4.378	3.883	7.484	4.204	3.901	3.870	1.388	3.940

Table 9.14 continued

	1	2	3	4	1	2	3	4
Variables Referring to Head								
Non-School Training	.234	.235	.241	.234	.423	.424	.428	.42
Age at Marriage				23.007				4.61
Physical Handicap	.176	.145	.123	.171	.380	.352	.329	.37
Union Member	.260	.309	.332	.331	.439	.462	.462	.463
Self-Employed	1.245	1.288	1.234	1.219	.614	.665	.618	.58
Job Tenure	96.291	109.018	106.266	107.156	99.101	100.290	96.695	101.529
Family Structure								
Single Female Present	.209	0	0	.175	.407	0	0	.38
Single Male Present	.080	0	0	.044	.271	0	0	.20
Number of Children	1.460	1.719	1.531	1.541	1.670	1.692	1.576	1.69
Age of Youngest Child	11.760	10.560	11.024	11.593	6.422	6.442	6.294	6.40
Attitudinal Variables								
Risk Aversion	5.114	5.260	5.359	5.193	1.594	1.580	1.527	1.54
Low Educational Goals for Children	1.146	1.295	1.223	1.219	1.270	1.249	1.234	1.27
Achievement Motivation	8.846	9.134	9.174	8.881	2.733	2.630	2.570	2.74
Community Variables								
Non-South Region	.720	.718	.702	.729	.449	.450	.458	.44
Distance to Nearest City	24.737	25.808	24.993	24.474	22.824	22.825	22.508	22.60
Unemployment Rate	5.751	5.735	5.627	5.734	2.156	2.168	2.183	2.17
Male Labor Shortage	4.032	4.020	4.006		.906	.908	.875	
Female Labor Shortage	4.406	4.374	4.324		1.318	1.319	1.238	
Typical Male Wage	2.286	2.295	2.281		.513	.523	.504	
Typical Female Wage	2.002	2.003	2.011		.349	.352	.350	
Other Income Variables								
Asset Income	362.369	391.159	317.868	333.400	1195.396	1220.198	1006.508	936.08
Taxable Income	9232.614	11214.430	12187.869	9618.922	7353.197	7413.952	5993.164	6908.70
Taxable Income 1/3	19.000	21.245	22.406	19.503	6.918	5.331	3.764	6.65
Ln Taxable Income	8.391	8.998	9.280	8.516	2.215	1.348	.559	2.11
Welfare	105.626	40.581	14.753	104.994	516.069	351.347	186.911	526.423
Other Transfer	410.526	325.908	218.485	363.276	1046.150	1001.445	687.544	983.709
Total Transfer	516.152	366.490	233.238	468.270	1162.292	1065.899	710.490	1108.907
Family Income	9748.766	11580.920	12421.107	10087.191	7046.671	7204.621	5910.982	6582.868
Family Income$^{1/3}$	20.229	21.812	22.608	20.620	4.986	4.349	3.570	4.726
Ln Family Income	8.915	9.184	9.316	8.992	.897	.699	.497	.812

Table 9.14 continued

	1	2	3	4	1	2	3	4
4 Year Averages								
1971 Male Adult Variables families with a male								
Weeks Worked				45.775				8.879
Wages				4.086				2.635
Ln Wages				1.273				.531
Earnings				8927.021				5257.577
Earnings$^{1/3}$				19.864				4.472
Ln Earnings				8.818				1.248
1971 Female Adult Variables for families with a female								
Weeks Worked				20.945				20.042
Wages				1.158				1.389
Ln Wages				.331				.539
Earnings				1868.630				2428.524
Earnings$^{1/3}$				8.402				7.025
Ln Earnings				4.925				3.572
Asset Income				313.171				858.930
Taxable Income				9463.625				6159.501
(Taxable Income$^{1/3}$)				19.546				6.038
(Taxable Income)$^{1/3}$				19.756				5.819
Ln Taxable Income				8.666				1.748
Welfare				100.458				500.837
Other Transfer				298.941				823.143
Total Transfer				399.398				921.971
Family Income				9863.023				5891.590
(Family Income)$^{1/3}$				20.623				4.210
Ln Family Income				9.011				.663

Sample 1 = all households, N=3160
Sample 2 = all households with male head and spouse, N=2245
Sample 3 = all households with employed male head and employed spouse, N=1134
Sample 4 = all households with no change in head or spouse between 1968 and 1972 and head not a student between 1968 and 1971, N=2005
(Samples 2 and 4 are contained in sample 1; sample 3 is contained in sample 2.)

Appendix: Table 9.15

Correlation Matrix for Components of 1971 Family Income (1967 dollars) and Unstandardized Bivariate Regression Coefficients (B) Predicting Family Incomes from Each Component for 2245 Households With a Male Non-military, Non-student Head Aged 25-64, Spouse Present and Complete Data on All Basic Variables.

	Male Earnings	Female Earnings	Assets	Welfare	Other Transfers	Total Taxable Income	Total Transfers	Total Family Income
Male Earnings	1.00000							
Female Earnings	-.02009	1.00000						
Assets	.25026	.00606	1.00000					
Welfare	-.13824	-.06174	-.01319	1.00000				
Other Trans. Inc.	-.25282	-.05879	.08340	.02096	1.00000			
Taxable Income	.93402	.30089	.39154	-.14612	-.23231	1.00000		
All Transfers	-.28223	-.07520	.07410	.34303	.94631	-.26550	1.00000	
Total Family Income	.91940	.29851	.41388	-.09960	-.09905	.98978	-.12527	1.00000
B	.994	.912	2.444	-2.082	-.713	.962	-.847	1.000

Table A2B: Variance-Covariance Matrix for Components of 1971 Family Income (1967 dollars) for 2245 Households with Complete Data on All Basic Variables (see Table 8) and with a Non-student, Non-military Head Aged 25-64.

	Male Earnings	Female Earnings	Assets	Welfare	Other Transfers	Total Taxable Income	Total Transfers	Total Family Income
Male Earnings	44445710							
Female Earnings	-315733	5557125						
Assets	2035808	17431	1488884					
Welfare	-317618	-50164	-5545	118780				
Other Trans. Inc.	-1687926	-138789	101912	7235	1002892			
Taxable Income	46165853	5258751	3542064	-373366	-1724824	54966683		
All Transfers	-2005554	-188955	96375	126015	1010129	-2098121	1136141	
Total Family Income	44160152	5069850	3638447	-247299	-714649	52868815	-961998	51906564

Appendix: Table 9.16

Correlation Matrix for Components of 1971 Family Income (1967 dollars) and Unstandardized Bivariate Regression Coefficients (B) Predicting Family Incomes from Each Component for 1134 Households With Employed Non-military, Non-student Male Head Aged 25-64 and Employed Spouse, and Complete Data on all Basic Variables

	Male Earnings	Female Earnings	Assets	Welfare	Other Transfers	Total Taxable Income	Total Transfers	Total Family Income
Male Earnings	1.00000							
Female Earnings	.11803	1.00000						
Assets	.14163	.07147	1.00000					
Welfare	-.10210	-.06570	.02495	1.00000				
Other Trans. Inc.	-.15010	-.07161	.02335	-.01111	1.00000			
Taxable Income	.89111	.52457	.31356	-.11506	-.14868	1.00000		
All Transfers	-.17212	-.08658	.01603	.25231	.96478	-.17415	1.00000	
Total Family Income	.88281	.52146	.31985	-.08633	-.03479	.99297	-.05637	1.00000
B	1.064	1.236	1.878	-2.730	-.299	.979	-.969	1.000

Table A3B: Variance-Covariance Matrix for Components of 1971 Family Income (1967 dollars) for 1134 Households with Complete Data on All Basic Variables (see Table 8) and with a Non-student, Non-military Head Aged 25-64

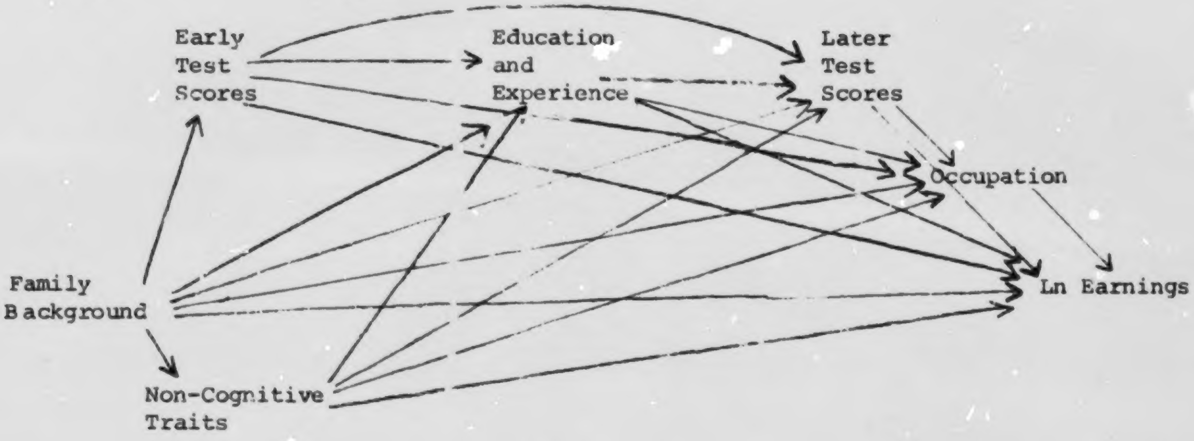
	Male Earnings	Female Earnings	Assets	Welfare	Other Transfers	Total Taxable Income	Total Transfers	Total Family Income
Male Earnings	24046643							
Female Earnings	1442982	6215592						
Assets	699036	179342	1013059					
Welfare	-93583	-30616	-4683	34937				
Other Trans. Inc.	-506068	-122748	16159	-1428	472717			
Taxable Income	26188747	7837921	1891447	-128892	-612646	35918016		
All Transfers	-599676	-153362	11463	33507	471289	-741546	504797	
Total Family Income	25589049	7684612	1902932	-95387	-141389	35176447	-236737	34939714

Chapter 10: Determinants of Individual Earnings: A Summary

By Mary Corcoran

To what extent do highly paid jobs go to the brightest, to the most educated, to the hardest working, or to the sons and daughters of the rich? Previous chapters have looked at the effects of each of these factors separately. This chapter summarizes and synthesizes the evidence presented in earlier chapters. Like previous chapters it assumes that family background, non-cognitive traits, test scores, education, work experience and occupational status can affect earnings in two ways -- indirectly, i.e., by affecting the level of other factors, and directly, i.e., independently of other factors. In sorting out these direct and indirect effects one needs to make assumptions about the causal order of the different factors. I use the same "life cycle" model as earlier chapters, which is shown in Figure 1.

Figure 1 - The Earnings Model


Early Test Scores
Education and Experience
Later Test Scores
Occupation
Family Background
Ln Earnings
Non-Cognitive Traits

I will concentrate on the determinants of Ln Earnings. Such a model predicts the percentage change in earnings for each unit change in another factor. We estimated the model pictured in Figure 1 for adult men aged 25 to 64 years. Whenever possible, we omitted zero earners, military officers and enlisted men, full-time students and institutionalized individuals. We began with linear and additive assumptions and then explored non-linearities and interactions.

1. Effects of Family Background

Demographic Background. Duncan, Featherman, and Duncan's (1972:43) tabulations for white non-farm males **aged** 25-64 in the OCG implied that father's education, father's occupation, and number of siblings explained between five and nine percent of the variance in 1961 income, depending on the age of the respondent. (R^2 was higher for those aged 35-54 than for those aged 25-34 or 55-64.) Jackson found that when he included blacks and farm born men, extended the list of background variables to include race, non-farm upbringing, non-Southern upbringing, and living with one's father at age 16, and dropped men with missing data, R^2 was 0.147 for 25 to 64 year old males. Predicting the logarithm of income instead of income raised R^2 to 0.166.

This OCG estimate of the influence of family background on Ln Earnings is somewhat larger than that obtained in national studies of men 25-64 during the 1970's. R^2's varied from 0.194 for the PA (1964) to 0.166 for OCG (1961) to 0.117 for PSID (1972) and 0.064 for NORC (1973). The influence of background appears to have declined between the early 1960's and early 1970's. Featherman and Hauser observed a similar trend in their analyses of the 1973 OCG replication.

These estimates are all biased downwards because they omit certain potentially important background variables and fail to measure others accurately. In Chapter 3, I estimate that including parental income, religion and ethnicity and eliminating measurement error might raise R^2 to 26 percent in 1961-1964 and 19 percent in 1971-1973.

Unmeasured Background. Even with perfect measurement, however, "demographic" characteristics may not pick up the full impact of coming from one family rather than another. If brothers exert no direct effect on one another, the interclass correlation between their earnings is equal to the percentage of variance we would be able to explain if we regressed earnings on all genetic and environmental factors that brothers have in common. Jencks et al (1972) estimated this correlation at 0.15, but their estimate was based on brothers' **resemblance on test** scores, education, and occupational prestige; / **they did not have data on** brothers' earnings. The NORC Brothers Survey yielded a correlation of 0.13 between the logarithms of brothers' earnings for 150 pairs of brothers with complete data. The correlation for men whose brothers NORC could not locate appears to be even lower. Nonetheless, the 95 percent confidence interval for this correlation in the population as a whole runs from -0.03 to 0.29. All the other data we have located on brothers' earnings suggest that the true correlation between brothers lies above the NORC point estimate rather than below it. Olneck's Kalamazoo data, for example, show that for 392 pairs of brothers raised in Kalamazoo, the correlation between the logarithms of brothers' earnings was 0.22. An independent study by John Brittain of the Brookings Institution found a correlation of 0.44 between the logarithms of earnings for a small sample of brothers whose parents had died in the Cleveland area. Paul Taubman reports that in a national sample of nearly 1,000 pairs of DZ twins and 1,000 pairs of MZ twins, the correlations between the logarithms of twins' earnings were 0.30 for DZ twins and 0.55 for MZ twins. One would expect DZ twins to end up somewhat more alike than ordinary siblings. This should be even more true for MZ twins. Taubman's results therefore suggest that the correlation between ordinary brothers' earnings is less than 0.30. Judging by the evidence now available, an estimate on the order 0.25 seems reasonable. Corrections for random reporting errors might raise the estimated value to 0.29 or 0.30 for Ln Earnings in one year. Using Jencks' figures for the transient component of earnings, the estimated correlation between brothers' lifetime earnings could be as high as 0.30/0.71 = 0.42.

If the observed correlations between brothers' earnings in a single year were on the order of 0.29, and if demographic background characteristics explained 19 percent of the true variance in earnings in the early 1970's, unmeasured background characteristics must have explained at least 10 percent. The unmeasured background characteristics that make brothers alike with respect to earnings are not the same as those that make brothers alike with respect to test scores, education, or occupational status. This means that parental "advantages" cannot be fully understood using one-dimensional concepts like socioeconomic status (SES). Parental attitudes and values probably play a role too, and those that maximize one outcome may not maximize others.

Mechanisms by which Background Affects Earnings. Employers do not appear to hire or promote workers on the basis of their demographic background. Controlling test scores and educational attainment makes the coefficients of father's education, father's occupation, number of siblings, and coming from a broken home extremely small and usually insignificant in our surveys. Southern birth and farm upbringing exert some effect even among men with similar test scores and education, but these effects become negligible once we control the size of the community and region in

The only demographic characteristic that consistently has a substantial effect on earnings with everything controlled is race. Race is, of course, not just a characteristic of the respondent's parents but a visible characteristic of the respondent himself. The fact that employers pay mature blacks less than whites with the same test scores and the same amount of education strongly implies that in this instance employers engaged in "pure" discrimination. **Alternatively, whites and non-whites may differ on some important but as yet unmeasured set of skills or qualifications.**

When we turn to unmeasured features of family background, the picture is quite different. Here, we find that even after controlling demographic background, test scores, educational attainment, and occupational status, brothers end up more alike with respect to earnings than one would expect. This could mean that brothers resemble each other with respect to other traits that employers value (e.g. attitudes and values). Or it could mean that brothers share certain values that affect their earnings independent of what employers are willing to pay (e.g. taste for leisure). Or it could mean that brothers affect one another directly.

Conclusions about Background.

1. Measured parental advantages are moderately related to men's earnings. The size of this relationship declined between 1962 and 1972. What we have called "demographic" background accounted for less than a fifth of the variance in Ln Earnings for mature men in 1973.

2. Most of the effect of measured background is indirect. Parental demographic characteristics exert considerable influence on children's test scores and educational attainments, which in turn influence adult earnings. The one exception to this is race. Blacks earn considerably less than whites with comparable schooling and test scores.

3. Parents' demographic characteristics do not capture the full

influence of family background on men's earnings. We could probably explain an additional 10 percent of the variance in Ln Earnings if we could measure all the background traits that make brothers alike.

4. Unmeasured parental characteristics exert considerable impact on sons' earnings, independently of test scores and educational attainment. The unmeasured characteristics that create these effects are almost uncorrelated with the measured advantages typically found to influence men's earnings.

2. Effects of Test Scores

Four of our surveys included some measure of cognitive skills, but each used a different test. Only one nationally representative sample (PSID) measured cognitive skills, and its measure was crude and unreliable. Two tests were given prior to school completion (Talent, Kalamazoo); one was given to young adults (Veterans), and one was given at the same time earnings data were collected (PSID). The Veterans survey underrepresented low-scoring and high-scoring men because of army recruiting procedures and because some army officers were not tested. All the Talent 28-year olds had completed eleventh grade so low-scoring individuals are undersampled and those with less than 11 years of school are not sampled at all. Since early test scores affect earnings partly by affecting the amount of education one acquires, systematically dropping either men with little education or men with a great deal of education will bias the reduced-form coefficient of test scores downward. It will also underestimate the mediating role played by education.

I will begin by discussing adolescent test scores and then will discuss adult test scores. I have standardized all cognitive test scores to a mean of 100 and a standard deviation of 15, so a one-point increase in test scores has roughly the same meaning across surveys.

Observed Associations for Adolescent Test Scores. A one-point increase in adolescent test scores was associated with an 1.1 percent increase in earnings for Kalamazoo men 35-59 and with an 0.6 percent increase in earnings for Talent 28 year olds. This spread in the magnitude of effects is likely caused by age differences. This hypothesis is supported by the fact that a one-point increase in AFQT scores raised earnings 0.6 percent for Veterans 25-29 and 1.2 percent for Veterans 30-34. The relationship between adolescent test scores and Ln Earnings is almost linear.

Effects with Background Controlled. Very little of the relationship between adolescent test scores and Ln Earnings arises because men with high test scores come from advantaged families. A fifteen point difference in Kalamazoo men's test scores is associated with a 17.2 percent difference in their earnings. If we compare men with similar measured backgrounds, a fifteen point test score difference results in a 15.1 percent earnings difference. If we compare brothers, a fifteen test score point difference is associated with a 17.1 percent difference in their earnings. **The pattern of results for Talent brothers is almost identical, though the unrepresentative character of this sample makes the findings inconclusive.**

Mechanisms by which Adolescent Scores Affect Earnings. Test scores affect earnings partly because men with high adolescent test scores tend to get more schooling and to enter higher status occupations than men with low scores, but that is not the whole story. In Kalamazoo, a 15 point difference in brothers' test scores is associated with a 13.8 percent difference in earnings if brothers have the same amount of schooling and an 11.4

percent difference if they also have the same occupational status. Results for Talent brothers are similar. Analyses of the full Talent sample show much weaker test score effects, but schooling and occupational status play a comparable role as intervening variables. No interaction between test scores and any background, age, education or experience variable was significant in both the Talent and Kalamazoo surveys. This suggests that one cannot account for the modest size of the correlation between test scores and earnings by saying that high scores are a necessary but not sufficient condition for high earnings. High scores are neither necessary nor sufficient. They are merely helpful.

Adult Scores. The Veterans and PSID surveys measure test scores after school completion. Adult test scores are likely to be affected by the amount of schooling men acquire, so they should predict Ln Earnings more accurately than do adolescent test scores. The only study that has test scores both before and after school completion for the same individuals fits this pattern. Fagerlind (1973) found that tests taken after school completion predict economic success much more accurately for Swedish men than do tests taken prior to school completion. But this pattern does not hold for our samples. Comparisons between our samples are suspect, however, because the underrepresentation of high-and low-scoring veterans and the unreliability of the PSID test attenuate the correlations involving adult test scores.

Adult test scores affect men's earnings in much the same way as do adolescent test scores. A one point increase in test scores increases earnings 1.8 percent for PSID men and 1.2 percent for Veterans 30-34. Controlling measured background advantages reduces these effects by about one-fourth to 1.3 percent for PSID men and to 0.9 percent for **Veterans** 30-34. Further controlling schooling and occupation reduces the impact of a one-point increase in adult test scores to a 0.6 percent increase in earnings for both PSID men and Veterans.

As with adolescent test scores, additive and linear assumptions adequately capture the influence of adult test scores on Ln Earnings. The Test Score^2 term was insignificant in both the PSID and Veterans regressions. No multiplicative interaction between test scores and other variables was significant in both the PSID and Veterans analyses.

Despite variations in populations, tests and timing of tests, cognitive test scores affected earnings in much the same way across surveys. Perhaps the biases balance one another out.

Conclusions about Test Scores

1. Men with high adolescent test scores earn more than men with low scores. Very little of this apparent advantage arises because men with high test scores are likely to come from favorable backgrounds. Less than half the effect of adolescent test scores on earnings after 30 is explained by the fact that high adolescent test scores lead to high educational attainment and high occupational status.

2. Men with high scores on tests taken as adults also earn more than men who get low scores on similar tests.

3. The economic benefits of high test scores increase with age, at least up to 30.

4. A one standard deviation increase in test performance is probably associated with a 20-30 percent increase in lifetime earnings with background controlled.1/

1/ The observed coefficients for mature men are 0.0086 for Kalamazoo brothers and 0.013 for PSID. The Kalamazoo value is low because of restricted variance in Ln Earnings the latter is low because the test is

3. Effects of Education on Earnings

Association of Schooling with Earnings. Table 10.2 shows regressions of Ln Earnings on education in eight surveys. Years of education accounted for 16 to 24 percent of the variance in Ln Earnings in the five large national surveys (OCG, PA, NLS, Census, PSID). The variations in R^2 are probably caused by sampling and measurement differences. Differences among very low earners account for a large fraction of the total variance in Ln Earnings. These differences are very weakly related to education. Eliminating them by grouping therefore reduces the unexplained variance and increases R^2. The PSID and PA surveys did not group earnings, but neither did they cover as many men with very low earnings as the other surveys. This, too, inflates R^2 2/

1/ (continued) unreliable. Correcting for unreliability could easily yield a coefficient of $0.013/(0.64)^{1/2} = 0.016$. A 15 point increase in test scores would then imply that earnings rose by $e^{(15)\ (.016)} -1 = 28$ percent. The coefficients for young men are much lower, so the lifetime estimate is a compromise.

2/ Another way to compare predictability across samples is to look at the S.D.'s of the residuals. These were: 0.794 in NLS, 0.740 in OCG, 0.658 in the Census, 0.674 in PSID, and 0.618 in PA. These S.D.'s represent the variation in Ln Earnings unaccounted for by education. Parnes men have far more variation in residual Ln Earnings than do men from other national surveys. This is an age effect; earnings of men 45 to 59 are more variable than earnings of men 25-64. PA men have less variance in residual Ln Earnings than do men from other national surveys. This is probably a sampling effect. The PA undersampled poor men--men whose differences in earnings are less explicable by education differences. The PSID falls between the PA and the Census in this respect. The OCG measured income not earnings, and grouped income, so it is not comparable to the others.

In addition, the Census earnings data may be less reliable than that in other surveys (see McClelland, Chapter 16). Education accounted for less than eight percent of the variance in Ln Earnings among veterans aged 30 to 34 and among Talent 28-year olds. This is both an age and a sampling effect; young men often have not yet realized the full economic benefits of their education, and the variance in education is restricted in both samples.

In the 1970 Census, schooling increased Ln Earnings in a non-linear fashion. Three education measures--total years of schooling, years of schooling past high school and BA--adequately capture these non-linear effects.[3/] The returns to four years of college were as great or greater than the returns to four years of high school in the Census analyses. But for college dropouts and for men who went on to graduate school, the percentage return to an extra year of education was only half as large as the return to an extra year of high school. (The reader should keep in mind that these are percentage returns. Actual dollar returns to a year of schooling increase after high school.) This suggests that there may be a "diploma effect." Attaining a BA appears to confer a substantial wage bonus on mature men. However, the years of higher education variable groups all kinds of post-high school education together, whether graduate or undergraduate, liberal arts or professional. Bartlett and Jencks found that graduate education increased earnings very little relative to a BA. This helps account for the low average benefits of a year of higher education and the substantial "diploma effect."

[3/] Bartlett and Jencks experimented with several measures before settling on the above. Their attempts are summarized in Appendix A.

Table 10.2
Regressions of Ln Earnings on Schooling and Work Experience

Sample	Sample Size	Years of Education	Higher Education	BA	Work Experience	Work Experience^2	Work Experience^3	R^2	S.D. of Residuals
OCG, a/	11,504	.1057	-.0924	.2743				.177	.743
(1961)		(.003)	(.011)	(.050)					
		.1128	-.0837	.2857	.0339	-.0005		.185	.740
		(.003)	(.011)	(.049)	(.003)	(.000)			
PA,	1,188	.1036	[-.0171]	[.0295]				.238	.618
(1964)		(.008)	(.029)	(.118)					
		.1069	[-.0108]	[.0541]	.0332	-.0005		.257	.611
		(.009)	(.029)	(.117)	(.006)	(.000)			
NLS 45-59 Year Olds	2,580	.1069	[-.0175]	[.0708]				.192	.794
(1966)		(.006)	(.026)	(.115)					
		.0950	[-.0123]	[.0744]	[.0382]	[-.0007]		.196	.793
		(.007)	(.026)	(.114)	(.031)	(.003)			
Census,	25,697	.0818	-.0255	.1110				.157	.658
(1969)		(.002)	(.006)	(.027)					
		.0849	-.0166	.1256	.0422	-.0007		.178	.650
		(.002)	(.006)	(.027)	(.002)	(.000)			
		.0864	-.0159	.1265	.0727	-.0021	.00002	.180	.649
		(.002)	(.006)	(.027)	(.004)	(.000)	(.000)		
PSID,	1,774	.1042	[-.0494]	.2314				.200	.674
(1971)		(.008)	(.024)	(.093)					
		.0841	[-.0129]	.1828	.0549	-.0012		.249	.653
		(.009)	(.023)	(.090)	(.006)	(.000)			
Veterans 30-34, year olds	803	.0532	[-.0012]	[.0433]				.098	.473
(1964)		(.012)	(.025)	(.094)					
		.0952	[-.0055]	[.0466]	.0393			.109	.471
		(.018)	(.025)	(.094)	(.013)				
Talent b/ 28 Year Olds	839	.0792	[-.0265] c	[.0645]				.168	.408
		(.018)	(.026)	(.083)					
(1972)		.0799	[-.0266] c	[.0666]	[.0066]			.168	.408
		(.018)	(.026)	(.083)	(.003)				

a/ All OCG runs look at Ln Income

b/ Talent runs examine Ln Weekly Earnings

c/ This is years of graduate school

[Coefficients in brackets are less than twice their standard error.]

Years of education, years of higher education and BA are highly correlated, so one needs a large sample to distinguish the effects of the three education variables. As a result, only the three largest surveys (OCG, PSID and the Census) had significant coefficients for all three education measures. But, coefficients of the three education variables were remarkably similar across surveys. The only two significant differences involved the two largest surveys, which have the smallest sampling errors. A year of elementary or secondary education raised 1970 Census respondent's earnings by only 8.2 percent, compared with 10.4 to 10.7 percent in the other large national surveys. And after controlling diploma effects, a year of higher education raised OCG respondents' incomes by only 1.4 percent compared to 5.6 to 9.4 percent for PA, PSID, NLS, and Census respondents. Returns to four years of higher education were, however, quite similar in all five surveys, raising earnings by a weighted average of 51 percent. Four years of high school raised earnings by a weighted average of 53 percent.

Effects with Experience Controlled. Schooling is not the only way to acquire useful economic skills. Skills may be learned on the job. Even when they are not, earnings in certain occupations are directly related to seniority. Staying out of the labor force to acquire education therefore imposes two costs: the loss of on-the-job training and/or seniority and loss of earnings during the period of schooling. Education coefficients subtract out the first cost unless years of experience are included in the equation.

... Lifetime earnings are probably best estimated from equations that control experience. Returns at any given age may, however, follow a quite different pattern from the lifetime average.

Controlling experience increased the estimated returns to education little if at all for older men. In younger samples, increases in returns to education were somewhat larger, since a year of lost experience costs more when total experience is low than when it is high.

Effects with Background and Test Scores Controlled

Controlling demographic background reduces the estimated effect of four years of high school on mature men's earnings or income from 53 to 36 percent. Controlling these same background characteristics reduces the estimated effect of four years of college from 51 to 41 percent. These estimates are weighted averages of results from the OCG, PA, PSID and NLS surveys. The OCG data on brothers' educational attainments suggest that controlling background characteristics would not alter the picture appreciably. The Kalamazoo brothers survey also supports this conclusion. The smaller NORC brothers survey implies that the estimated benefits of education are not changed when one controls background, but this aberrant finding is probably a byproduct of random sampling error.

The Kalamazoo results indicate that another quarter of the apparent influence of schooling on Ln Earnings arises because men with high adolescent test scores tend to get more schooling. An extra year of schooling is associated with a 5.1 percent earnings advantage over one's brother in Kalamazoo. This drops to 3.1 percent when differences between brothers' test scores are controlled. Overall, about half the apparent benefit of schooling disappears when family background and test scores are controlled in the Kalamazoo sample. The reduction in effects of schooling is lower for Talent brothers, but they are only 28. These findings suggest that for national samples of 25-64 year old men, the return to an extra year of schooling is between 4 and 8 percent, with the low end of the range more plausible than the high end.

Percentage returns to formal education did not vary in any consistent way from one subpopulation to another. Returns appeared roughly similar for whites and non-whites, for men with white-collar, blue-collar, and farm fathers, for men with high, medium and low test scores, and for men aged 25-34, 35-44, 45-54 and 55-64. This implies, of course, that absolute dollar benefits were higher for whites, for men with white collar fathers, for men with high test scores, and for men 35-54.

We also tested for interactions between education, experience and other variables by creating multiplicative interaction terms. No interaction involving education or experience was significant and had the same sign in more than one survey. This is not surprising, since samples other than the OCG and the Census are fairly small, and the different interaction terms are highly correlated with one another and with independent variables. A more useful test of consistency across samples is to ask whether an interaction that was significant in one survey would be significant (or at least have the right sign) if it were the first interaction added to the additive equation in other surveys. I asked this question for each of the six interactions involving education or experience which was significant in at least one survey other than the Census. None of these interactions had the same sign in OCG, PA, NLS, PSID, and Census.

Mechanisms by which Education Affects Earnings

Many people assume that education influences a man's earning potential because it provides him with intellectual skills that employers value.

To test this theory we would need to know what cognitive skills employers value. We have very little evidence on this score. But we can ask whether education provides men with general cognitive skills that enhance earnings in all occupations. To answer this question properly we would need test scores both before and after school completion. Then we could estimate how changes in test scores caused by different amounts of schooling affected earnings. We do not have any such data. Instead we have two samples with adult test scores and two samples with adolescent test scores. If schooling increases earnings by increasing general cognitive skills, then controlling adult test scores should reduce the apparent effects of schooling on Ln Earnings more than controlling adolescent test scores reduced schooling effects. Fagerlind's Swedish data supports this hypothesis. Our data do not, but that is probably because our two samples with adult scores suffer from more serious biases than our two samples with adolescent scores.

Both Kalamazoo and Talent obtained a measure of grades in high school. If schools increase men's earnings by improving skills, and if school grades measure economically useful skills, then men who perform well in school should also have a better chance of doing well economically. Even if grades do not measure the acquisition of skills, they might measure how hard an individual works or how well he adapts to institutional norms. Yet for Kalamazoo men aged 35 to 59 years and for Talent 28-year olds, grades have no significant effects on earnings once education is controlled. This is also true in Sewell and Hauser's Wisconsin data. Evidently high grades improve earnings by improving chances of acquiring more schooling, but they are not proxies for traits that employers find valuable regardless of schooling.

In large national samples, secondary education increased earnings in good part because it provided access to high status occupations with high average earnings. With demographic background, test scores, and experience controlled, the earnings differential between men with 12 rather than 8 years of schooling is 23-24 percent in the PSID, Kalamazoo, and Veterans samples. Controlling PSID's crude occupational status measure reduces the earnings differential to 18 percent. Controlling a more refined occupational status measure reduces the differential to about 9 percent in both the Veterans and Kalamazoo samples.

The pattern at the college level is less consistent. With demographic background, test scores, and experience controlled, the earnings differential between men with college degrees and men with only a high school diploma is 46 percent in PSID vs 29 percent for Veterans and 22 percent in Kalamazoo. Controlling the crude PSID occupational measure lowers the differential from 46 to 23 percent. Controlling the more refined measure lowers the differential to 10 percent for Veterans and 6 percent in Kalamazoo.

Non-Academic Training. The PA, NLS, Census and PSID also asked respondents if they had had any education outside a regular school or college. Unfortunately, the questions and the results varied from survey to survey, so generalizations are difficult.

The PA asked high school graduates who had not attended college whether they had had any non-academic training. 46 percent said they had. Those men constitute 16 percent of the total sample. Their earnings did not differ significantly from the earnings of high school graduates without non-academic training, but this could be because of random sampling error.

The NLS asked all its 45-59 year old respondents whether they had had any non-academic training. 49 percent said they had. After controlling demographic background, years of formal education, years of experience, and weeks worked, NLS respondents with non-academic training earned 10 percent more than those who lacked such training. This difference became insignificant once we controlled occupational status. This could mean that vocational training enhances earnings by helping men enter highly paid occupations. Alternatively, it could mean that men in highly paid occupations have access to more vocational training. Unfortunately, NLS did not collect data on the date of the non-academic training, so we cannot say whether training preceded entry into one's current occupation or vice versa.

PSID asked respondents about all training outside the regular school system, including vocational schooling, military training, on-the-job training, and formal apprenticeship. A quarter of all PSID respondents reported some form of training. These men earned 11 percent more than others with the same demographic background, years of formal education, and years of experience. Their advantage fell from 11 to 7 percent when we controlled weeks worked, and to 6 percent when we controlled broad occupational group.

The Census asked respondents whether they had ever completed a vocational training program and, if so, in what field. This could include vocational training received in high school as well as in less formal situations. About 28 percent of all Census respondents said they had completed a vocational training program. For men with equal schooling and experience, training in crafts, trades, business or engineering increased wages while training in agriculture decreased wages. When occupation was controlled, returns to training programs in engineering dropped from 16 percent to 5 percent and returns to business training dropped from 5 to -4 percent. Controlling weeks worked (in 1969) increased returns to engineering training from 5 to 17 percent.

College Quality

The PA ranked colleges into five categories: very highly selective, highly selective, selective, non-selective, and unaccredited. Holding family background and years of education up through college constant, attending any sort of selective college raised earnings by 28 percent. The differences between selective, highly selective and very highly selective colleges were not significant. Controlling years of graduate school did not alter this. Nor did controlling bread occupational categories or weeks worked.

Conclusions About Effects of Education

(1) Controlling measured background reduces the earnings advantage of four years of high school from 53 to 36 percent and that of four years of college from 51 to 41 percent. In those surveys with test scores, controlling both background and test scores reduced the apparent advantage of extra education even further. This suggests that estimates of returns to schooling which do not control for family background and test scores will be inflated, perhaps to as much as double the true level.

(2) Schooling affects earnings in part by affecting men's occupational status. This is particularly true of higher education. When measures of occupational status were added to the regressions of Ln Earnings on family background and experience, returns to four years of high school dropped from 36 to 25 percent and returns to four years of college dropped from 41 to 13 percent. **The apparant returns to education for those in a given occupation fell even further when we controlled test scores, although, the Veterans and Kalamazoo samples are the only ones with good tests detailed occupational categories, and respondents over 30. Four years of either school or college raised earnings by 6-10 percent with background, test scores, and occupational status controlled in these two samples. This suggests that education boosts earnings mainly by providing access to lucrative occupations.**

(3) With family background and education controlled, attending a selective college increases earnings for men by about 26 percent.

(4) Vocational training is positively associated with earnings; but it is unclear whether training increases men's earnings potential or whether high-status occupations pay more and also give more training.

(5) High school grades have no effect on men's earnings once years of education are controlled.

(6) With background and experience controlled, the effects of education and experience on Ln Earnings do not vary consistently by age, race, father's occupation, or test score in our samples.

4. Effects of Occupational Status

The status of one's occupation substantially affects earnings. The zero-order correlations between one's Duncan score and Ln Earnings range between 0.400 and 0.446, which means that occupational status accounts for 16 to 22 percent of the variation in Ln Earnings in our surveys. Adding occupation to the regression of Ln Earnings on family background, education and experience raises R^2 by 0.02 to 0.04. A ten-point increase in Duncan score was associated with approximately a 9.1 percent increase in income for OCG men with equal education, family background, and experience and with a 9.4 percent increase in earnings for NLS respondents equal in education, family background, work experience and weeks worked last year. Effects of occupational status were somewhat smaller for PA and PSID respondents, presumably because the PA and PSID coded only broad occupational categories.

Comparing effects of occupation across surveys is difficult, since the age of the respondents and the occupational classification scheme vary from survey to survey, but certain consistent patterns emerge. Returns to occupation are stronger for older men. Returns to occupation drop when weeks worked are controlled, suggesting that occupation affects earnings partly by affecting the number of weeks worked per year. Even when men are equal in family background, cognitive ability, education, work experience and weeks worked, however, the status of a man's occupation strongly and directly affects his wage. Moreover, education affects earnings in large part because it increases access to high status occupations.

5. Effects of Not Working

Some men work part of the year: others work all year. Clearly the number of weeks worked per year will influence annual wages. "Dual labor market" economists argue that high risk of unemployment is a characteristic of certain jobs and treat weeks worked as an involuntary aspect of employment. Other economists argue that there is a great deal of voluntary non-employment; men often work less than a full year because they are in school, because of sickness, or because of voluntary retirement. In these cases , non-employment is a proxy for individual, not job, characteristics.

The PA, NLS, Census and PSID asked respondents how many weeks they worked in the last year. With background, education, and occupation controlled, weeks worked has a coefficient of approximately 1.00 in the PA, Census and PSID equations predicting Ln Earnings. The coefficient is about 0.75 in NLS. This means that there is no partial correlation between weeks worked and weekly wages for PA, Census and PSID respondents, once background, education, and occupation are controlled. As might be expected, Ln Weeks Worked substantially increases the predictability of Ln Earnings, increasing R^2 by 0.13 for PA and PSID and 0.20 for the Census. The increase in R^2's was less for NLS respondents, perhaps because there is less variability in the number of weeks worked per year among older men.

Weeks Worked also varies less and explains less in years with relatively full employment.

6. Overall Explanatory Power of Personal Characteristics

It seems reasonable to ask how much of the variation in men's earnings can be explained by variations in their characteristics when they enter the labor force, i.e. their family background, their adolescent test scores and personality traits, their education, and their age or experience. Race, region of birth, education and work experience explain 21 percent of the variance in Ln Earnings among Census respondents. These same characteristics plus demographic background traits explain 24 percent of the variance in Ln Income among OCG respondents. These same variables (without father's occupation) account for 33 percent of the variance in Ln Earnings among PA men. They account for 29 percent of the variance in Ln Earnings among PSID men.

The explanatory power of individual traits would increase if we had fuller and more accurate measures. Adding the unmeasured characteristics that account for brothers' similarity, for example, would probably increase R^2 by around 0.10, though this estimate has a substantial margin of error. (The true value could easily be 0.05 or 0.15.) In both the Veterans and Kalamazoo surveys, adding test scores prior to entering the labor market to the regression of Ln Earnings on demographic background, education and experience raised R^2's by 0.02. Mueser's preliminary analyses of Talent 28-year olds suggest that self-assessments of personality traits in adolescence increase R^2's by another 0.02. These traits may, however, help account for resemblance between brothers, so the combined effect of unmeasured background and personality traits may be less than the sum of their separate effects. Adding unmeasured background traits, cognitive skills and personality traits might therefore raise R^2 by something like 0.10 + 0.02 + 0.01 = 0.13. Our lowest plausible R^2 is 0.24 + 0.05 + 0.02 + 0.01 = 0.32 for OCG, while our highest plausible value is 0.33 + 0.15 + 0.02 + 0.02 = 0.52 for PA.

Measurement errors also lower estimates of R^2's. As much as 12 percent of the variance in SRC earnings reports may be due to random error in measuring earnings. This implies that the true R^2 could be 0.52/0.88 = 0.59. Correcting for errors in measuring background test scores, and education might raise R^2 as high as 0.65. Analogous corrections in Census/CPS data imply values of R^2 of at least 0.43. This is a considerable margin of error. Still, we can say that something like half the variance in Ln Earnings among men 25-64 is probably traceable to factors that are in principle measurable prior to their entering the labor market.

Jencks suggests that even after correcting for error something like a quarter of the variance in these individuals' annual earnings is probably accounted for by interannual fluctuations around each individual's long-term mean. This estimate is based on eight years of PSID data. One important factor in these fluctuations is variation in the number of weeks men work per year. It is not clear to what extent these variations reflect temporal changes in men's desire to work and to what extent they reflect changes in macroeconomic conditions.

Conclusions

Initially we asked "Who gets the highly paid jobs?" The answers have interesting implications for those concerned about income inequality and/or equal opportunity. Background exerts a much larger influence on earnings than past research suggests, perhaps accounting for as much as 30 percent of the variance in one year's earnings and as much as 42 percent of the variance in earnings over an individual's lifetime. Even more surprising, there seem to be as yet unidentified aspects of background that do not affect a man's earnings by affecting his test scores or education. If we define inequality of opportunity as the extent to which being born to one set of parents rather than another determines life chances, equal opportunity is still a long way from being realized.

A man's qualifications, as measured by test scores, education and training also influence his earnings, but not so strongly as some have argued. While high test scores moderately increased expected earnings, high scores were neither necessary nor sufficient to obtain high earnings. Men with lots of schooling earn more than men with less schooling, but more than half this influence arises because men with privileged backgrounds and high test scores both get more schooling and earn more money. Furthermore, a large part of the remaining effect occurs because education increases men's chances of working in highly paid occupations. Indeed, education has very little effect on the earnings of men who If education influences earnings by increasing productivity, this effect operates in large part through occupational selection, either by workers or by employers.

Finally, our results suggest that workers' characteristics when they enter the labor force explain 45 to 65 percent of the total variance in earnings among men aged 25 to 64. The sources of the remaining variance have yet to be fully explored.

Chapter 11

Measures of Economic Success

by Christopher Jencks

This chapter investigates the relative merits of several alternative measures of economic success. It begins by looking at various systems for ranking occupations. Then it examines various ways of scaling earnings. It concludes with a discussion of the relative importance of occupational status and earnings in determining overall well-being or "utility." Chapters 13 and 16 discuss the reliability of these measures.

Chapter 11

1. Occupational Status

Measuring occupational status involves two distinct steps: assigning each respondent to an occupational group and assigning a status score to each occupational group. The first task is very difficult. The second is easy, at least by comparison.

(a) Occupational Classification Schemes.

One problem with assigning workers to occupational groups is that there is no social consensus about how one should aggregate jobs into occupations. Many people do not think of themselves as having an occupation at all. They simply think of themselves as holding a job. If you ask them what they do, they tell you who they work for, not what they do in their job. As a result, surveys that ask people to name their occupation get a large number of unclassifiable responses.

The Census Bureau and other survey organizations concerned with accurate occupational classification respond to this difficulty by asking people both who they work for and what they do in their jobs. The Census Bureau then uses this information to assign the respondent to one of 441 occupational categories. For many purposes, however, it aggregates these 441 "detailed" (or "3-digit") categories into 10 to 12 "broad" categories. Many survey organizations, including the Survey Research Center at Michigan, save time and money be coding respondents in these broad categories to begin with. This poses a number of problems that we discuss below.

Both the "broad" and the "detailed" Census categories differentiate jobs along many different dimensions simultaneously. The most important dimension seems to be the skills and information required to perform competently in a job. But jobs requiring identical activities may,be assigned to different occupations if they involve working for a different sort of employer, require different paper credentials, take place in different physical settings, or have traditionally recruited different sorts of workers. The Census Bureau has never tried to justify its occupational categories either conceptually or empirically. Nor has the Bureau tried to assign specific weights to the different factors it considers when deciding which jobs to distinguish and which ones to lump together. Thus there is no a priori reason for believing that the Bureau's classification scheme tells anything whatever about men's jobs. In fact, however, it tells us quite a lot.

A good occupational classification scheme should group jobs that have something in common. What they should have in common is clearly a matter for debate. But one plausible hypothesis is that a good scheme should group together workers whom employers regard as relatively interchangeable. One obvious way of measuring whether a particular classification scheme succeeds in this respect is to see whether employers hiring workers for a given job typically look for applicants whose previous job was in the same occupation. Applying this standard to the Census classification scheme, one finds that among men 18 and over who changed employers in 1972, only 43 percent remained in the same detailed occupational category.[1] This means that 57 percent of all jobs filled by experienced workers were filled by someone whose previous job had been in a different occupation. In some cases, of course, the new recruit may have had experience in the relevant occupation at an earlier stage in life. In other cases, workers with experience in the relevant occupation may have been in short supply. In most cases, however, the employer in question must have felt that a job in a different occupation was as relevant as a job in the same occupation. In a few cases this may have involved rational "job ladders," in which salesmen are recruited into management in the same company, or semi-skilled workers become foremen. This does not seem to be the usual pattern, however, since after the age of 30 moves are almost as likely to involve a decline in occupational status as an increase.

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[1] Byrne (1975), Table H.

The fact that 43 percent of men who changed employers remained in the same occupation suggests that jobs in some detailed Census categories must resemble each other far more than random jobs do. Unfortunately, we cannot say how many detailed occupational categories meet this requirement or how well they do so. It could be that 200 of the 441 detailed categories cover jobs that almost always recruit from within the occupation, while the other 241 occupations include jobs that recruit randomly. Or it could be that all detailed occupational categories recruit about 43 percent of their experienced workers internally. We know, however, that occupations are not exactly alike with respect to recruiting patterns. Table 11.1 shows the percentage of those remaining in the same detailed occupational category after changing employers. The table only presents these data broken down by broad occupational categories, because there are not enough cases in most detailed categories to yield stable results. It suggests that when employers hire professionals and craftsmen, they look for men with previous experience in the same occupation. When they hire for clerical jobs or for jobs that merely involve operating machinery, they are much less likely to feel that previous experience in the same occupation is important. They therefore recruit men from a variety of previous occupations. One would like to have comparable percentages for each of the detailed occupational categories within these broad categories, since there is no good reason to suppose that just because physicists and school teachers are both classified as "professional" workers, their rates of internal recruitment are the same. Unfortunately, such data are not readily available.[2/]

The broad categories are less meaningful in recruiting terms. The Bureau creates broad categories by grouping detailed occupations that it thinks have something in common. One can estimate how much these detailed occupations have in common by asking how often a worker who changes his detailed occupational category remains in the same broad category. If one were to aggregate random detailed occupations into broad categories of the same size as those used by the Census, 12 percent of all individuals who changed detailed occupations would remain in the same broad occupation by chance alone.[3/] In fact, 20 percent of those who change

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2/ The 1970 Census provides data on the percentage of men in each occupation in 1970 who had been in the same occupation in 1965. But it does not allow us to distinguish men who were still in the same occupation because they were still in the same job from men who were still in the same occupation despite having changed jobs. The latter statistic appears more relevant for present purposes, though the former is also important. In order to obtain statistics on detailed Census occupations, one would need access to the CPS data described by Saben (1967) or Byrne (1975).

3/ The proportion who would remain in the same broad occupational category if these categories were simply random aggregations of detailed occupational categories is $\sum p_i^2$, where p_i is the proportion of all those who changed detailed occupations falling in broad occupation group i. (This formula assumes that p_i is stable over the period under study, which is essentially true.) The value in the text is derived from Byrne (1975, Table 4).

Table 11.1

PERCENTAGE OF MEN 18 AND OVER IN 1972 REMAINING IN THE

SAME DETAILED OCCUPATIONAL GROUP AFTER CHANGING EMPLOYERS
(SHOWN BY BROAD OCCUPATIONAL GROUP)

Broad Occupational Group in 1972	Percentage
Professional, Technical, and Kindred	57.9
Farm Laborers	57.4
Craftsmen & Kindred	52.9
Managers & Administrators	45.5
Transportation Equipment Operatives	43.8
Service Workers	38.4
Sales Workers	37.3
Non-Farm Laborers	30.6
Clerical Workers	30.1
Non-Transportation Operatives	29.8

Source: Byrne (1975), Table H. The tabulations come from Current Population Survey data collected in January, 1973. The percentages are for men 18 and over in 1973 who reported a civilian occupation for both January 1972 and January 1973, and who reported having changed employers during the interval.

The broad occupational classification in Byrne's tables also includes private household workers and farm owners and managers, but there were not enough individuals in these two groups to provide reliable estimates of the percentage remaining in the same occupation when changing employers.

detailed occupational categories remain in the sam[illegible]d categories.[4] This suggests that broad Census categories are not jus[illegible] random aggregations of detailed categories. But the detailed occupations that fall in the same broad categories have only (20-12)/(100-12) = 9 percent more in common than detailed occupations that fall in different broad categories.

For all its flaws, the Census Bureau's occupational classification scheme is the only one in widespread current use. While the Bureau's scheme clearly aggregates some jobs that have little in common, it still does better than chance. We have therefore taken advantage of the detailed Census occupational classification wherever it was available. We used the broad groupings in SRC data, since that is all SRC provides.

(b) The Duncan Scale

Once a researcher has assigned every individual to an occupation, assigning each occupation to a status group is relatively simple. The best currently available system appears to be that of Duncan (1961). His system uses the detailed Census occupational categories for 1950. He assigned each 1950 occupational category a status score based on the percentage of men in the occupation who had 12 or more years of schooling in 1950 and the percentage of men with 1949 incomes of $3500 or more. He gave these two percentages roughly equal weight. Their weighted sum varied from 0 to 96. The distribution was skewed, with a long tail to the right, though it is less skewed today than it was a generation ago. The 1970 mean for men 25-64 was 41, with a standard deviation of 25.

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[4] Byrne, (1975:56).

We could update this scoring system using more recent data on the educational requirements and economic rewards of different occupations. So far as I know, nobody has actually done this, so the empirical consequences are unknown. I would not expect updating to make much difference. The educational selectivity and relative economic rewards of occupations changed very little from 1940 to 1950 (Hodge,1961) It would be astonishing if they changed much between 1950 and 1970. In any event, updating the classification scheme would make it much more difficult to compare surveys done at different times. We therefore decided to stick with Duncan's original scores. As we shall see, they are quite serviceable.

We describe what Duncan measured as occupational "status." It should not be confused with what NORC measures when it asks respondents to rank occupations in terms of general standing or desirability.[5] Most investigators call this occupational "prestige," though the distinction between "status" and "prestige" is obviously arbitrary and is not universally made. Until the mid-1960's NORC's "prestige" ratings were only available for a handful of occupations. Survey researchers estimated the prestige of other occupations subjectively. This procedure was not very reliable. Duncan originally developed his scoring system to remedy this difficulty. He began with data from a 1947 NORC survey that had asked respondents to rate more than 90 occupations as "Excellent," "Good," "Average," "Fair," or "Poor." He found 45 occupations on the NORC list that corresponded closely to the detailed Census classification. He then predicted the percentage of respondents rating these occupations "Good" or "Excellent," using 1950 Census data on the percentage of men in

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[5] See Reiss (1961), Siegel (1971), Goldthorpe and Hope (1974).

each occupation with 12 or more years of school and the percentage earning $3500 or more. Having estimated this equation for the 45 detailed Census occupations on which NORC had prestige rankings, he used his equation to predict prestige for all other detailed Census occupational groups.

Taken at face value, Duncan's scale was merely a fallible device for estimating prestige. But when Hodge, Siegel and Rossi collected actual prestige scores for nearly 300 detailed Census categories in 1964 (Siegel, 1971), it became clear that Duncan had stumbled onto something more fundamental. When Duncan ranked survey respondents using both Duncan scores and Hodge-Siegel scores, their Duncan scores almost invariably predicted their **other characteristics (and their children's characteristics) better than their** Hodge-Siegel score did. The correlation between fathers' and sons' Duncan scores, for example, was consistently higher than the correlation between their Hodge-Siegel scores. The difference was large enough so that with Duncan scores controlled, the partial correlation between fathers' and sons' Hodge-Siegel scores was zero.[6/] This suggests that what is really important about a man's occupation for purposes of intergenerational mobility is the occupation's educational requirements and economic rewards. Prestige scores have predictive power only insofar as they correlate with these two occupational characteristics. Insofar as prestige depends on other factors, its predictive power is negligible. It seems fair to conclude that when occupational position is an independent variable, prestige scores are fallible Duncan scores, not the other way round.[7/]

[6/] See Duncan, Featherman, and Duncan (1972); also Featherman, Jones, and Hauser (1975).

[7/] For further analysis see Klatsky and Hodge (1971), Featherman, Jones, and Hauser (1975), and the sources cited there.

Confronted with evidence of this kind a sceptic may wonder whether the respondent's Duncan score might not, in turn, merely be a fallible proxy for the respondent's own education and income. It is not. A father's Duncan score has a significant effect on his son's education, occupation, and income, for example, independent of the father's education and income (Sewell and Hauser, 1975; Bielby et al, 1976). This could conceivably be because of errors in measuring father's education and parental income, but that seems unlikely. Certainly conventional wisdom suggests that a man's work shapes his life in ways independent of his prior education or his consumption level. The mean education and income of men engaged in a given line of work tell us something about all those who do it, independent of any given worker's own traits. The status of an individual's occupation is thus important in its own right, not just as a proxy for his education or income. This situation is in some ways analagous to the "contextual effects " of schools, whereby the mean ability or aspirations of the students in a given school affect individual students' chances of attending college, even with their own initial ability and aspirations controlled.

This does not, of course, mean that Duncan's scaling procedure was perfect. Mueser has convinced me that a rigorous defense of the Duncan scale would require us to rank occupations according to a wide range of criteria, including not only educational requirements and economic rewards but social prestige, training requirements, cognitive difficulty, stability of employment, degree of authority over others, degree of autonomy and so on. We would also have to obtain these data for each individual in a given occupation. We would then have to use both the occupational means and the individual data to predict other characteristics of individual respondents (or their children). If the various occupational means had the same relative weights when predicting all outcomes that interested us, we could justify constructing a single index of occupational position. If the weights differed significantly, no single index would be defensible. If occupational characteristics other than educational requirements and economic rewards were significant, or if these two occupational characteristics had different weights, our scale would differ from Duncan's.

We have no such data, so we cannot argue that the Duncan scale is a "perfect" system for ranking occupations. It is clearly superior to the most widely used alternative, namely the Hodge-Siegel-Rossi prestige scale, but we cannot say for sure that it is better than more traditional scales, such as the one developed by Hollingshead (Hollingshead and Redlich, 1958). Still less can we claim that the Duncan scale is superior to alternative scales that start by grouping jobs into different occupations than those the Census uses.

If one defines a father's occupational status in terms of his sons' life chances, the Duncan scale slightly overestimates the status of farm fathers during the first third of the 20th century. It slightly underestimates the status of white collar fathers during this same era. As a result, adding dummies for having a farm father and a white collar father consistently improves our ability to predict a son's life chances. But with Duncan score controlled, the residual effects of farm origins are smaller for men 25-64 in 1973 than in 1962. The effects of both farm upbringing and a white collar father are also smaller for men born between 1927 and 1936 than for older men in OCG. By 1950, when Duncan's data were collected, the residual effects of having a farm or white collar father might well have been zero. Rather than viewing the significance of the dummies as evidence of a flaw in Duncan's estimation procedure, then, one might view them as evidence that no occupational ranking is completely invariant over time. In any event, the implied flaws in the Duncan scale are not large.

Our next question about occupational status was whether Duncan scores constituted an interval scale in the sense that, say, income does. Income constitutes an interval scale because a $1000 change in income affects purchasing power by the same amount no matter where it occurs on the scale. This means, for example, that we can average two people's incomes and get meaningful results. But status does not buy anything tangible. How, then, might we tell whether the difference between men with Duncan scores of 10 and 20 is "the same" as the difference between men with Duncan scores of 80 and 90? One possibility would be to define status as a non-monetary reward of holding a given job. We could then investigate the "price" people were willing to pay for additional status by asking them how much income they would be willing to sacrifice in order to raise their occupational position by a given amount. Men who had spent many years in their present occupation might, of course, be less willing to make sacrifices that required them to change occupations. But one could get around this problem by asking young respondents to specify the incomes that would make jobs in different occupations equally attractive. Thus if two hypothetical men had Duncan scores of 10 and 20, respondents might typically feel that if the man with a score of 10 earned $1000 more than the man with a score of 20, the two men were equally well off. If it also took a $1000 income advantage to equalize the overall well-being of men with Duncan scores of 80 and 90, we could say that each point on the Duncan scale was "worth" $100 and that the scale had interval properties. Rainwater has actually collected data that would allow one to conduct crude tests of this sort, but we did not have the time or resources to analyze the data in the manner suggested. So far as I know, no one else has attempted such an analysis either. As a result, we have no general evidence that Duncan scores constitute an interval scale.

The Duncan scale yields scores whose distribution is significantly skewed, with a long tail to the right. If the "true" distribution were normal, Duncan's skewed distribution would overestimate the magnitude of status differences near the top of the scale and underestimate differences near the bottom of the scale. I tested this hypothesis in two ways. First, I looked at the relationship of a son's occupational status to his father's occupational status. Second, I looked at the relationship of a man's occupational status in 1965 to his status in 1970.

If the true distribution of fathers' scores were symmetrical, and if the true effects were linear, using a skewed scale would make the effects of a father's score on his son's score appear to diminish as one moved from the bottom of the scale to the top. This pattern recurred consistently in most of our samples. In the OCG, for example, the estimated curve is such that men whose fathers scored 20 on the Duncan scale outrank men whose fathers scored 10 by six points, whereas men whose fathers scored 90 outrank men whose fathers scored 80 by only three points.[8/] Such a deviation from linearity is not large enough to affect R^2 appreciably, but it does suggest that the effects of a father's occupational status may not be as skewed as Duncan's scoring system implies. If the underlying distribution of occupational status were really normal, the standard deviation of current occupational status for respondents with high status fathers should also exceed the standard deviation for men with low status fathers. This pattern appears in the OCG and NLS, which classify fathers by detailed Census categories, though not in the PSID, which uses broad categories. In OCG, fathers with Duncan scores of 9 or less have sons whose mean Duncan score is 28.5 with a standard deviation of 21.2. Fathers with Duncan scores of 90 or more have sons whose mean Duncan score is 62.7 with a standard deviation of 26.8.

Confronted with this evidence, we thought it worthwhile to experiment with a power transformation. The square root of Duncan's original values has an almost normal distribution. But taking square roots lowered the correlation between father's occupation and son's occupation for OCG men aged 25-64 from 0.412 to 0.403.[9/] The correlations of both father's occupation and respondent's occupation with the respondent's other traits were also depressed. This suggests that the skewness of the original scale

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[8/] This result was calculated from Appendix B, Table 4B.

[9/] This comparison was made for a subsample that excluded men with no father at home and men with incomplete data on other items. The 5/6 represents reality somewhat more accurately than a normally distributed scale would.

This conclusion held with even greater force when I looked at occupational mobility within a single generation. If a unit change in a man's Duncan score meant less at the top of the scale than at the bottom, one would expect occupational movements by men at the top of the scale to look "longer" than movements at the bottom of the scale. In order to test this hypothesis, I examined the absolute change in Duncan score of Census respondents aged 25-64 in 1970 who changed their detailed Census occupational category between 1965 and 1970. The mean shift was 16 points. The size of this shift correlated only 0.04 with the respondent's 1965 Duncan score. This suggests that the data are almost homoscedastic.

These investigations do not ensure the Duncan scale will have interval properties in every conceivable context. They do suggest that the skewness of the scale does not distort reality very seriously. This assumption is supported by the fact that while traits that boost mean Duncan scores tend to boost the variance as well, the increase is neither large nor consistent.[10] We therefore proceeded on the assumption that the scale had roughly interval properties.

2. Earnings

(a) Measurement Problems

At first glance measuring earnings may seem relatively straightforward: one simply asks the respondent how much he earns and codes the answer. But life is rarely so simple. First, there is the problem of how to define earnings. Second, there is the problem of choosing an accounting period. Third, there is the problem of what to do if either the respondent or the survey organization has "rounded" the answers in some way. Fourth, there is the problem of what to do if the survey organization asked about income rather than earnings.

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[10] See Tables 3A, 4A, 5A, 6A, 7A, 8A, and 9A in the Appendices.

The Census Bureau defines earnings as all income from wages, salary, or self-employment. It excludes income from assets (dividends, interest, rent) or transfer payments (unemployment insurance, social security, private pensions, gifts, and so forth). This definition has the virtue of simplicity, and we have tried to use it wherever possible. Economists dislike it because income from self-employment includes not only returns to labor but returns to capital investment as well. Unfortunately, one never knows how much self-employment income is really "labor income" and how much is "asset income." A farmer who owns land and machinery worth $500,000, for example, could make at least $25,000 a year without lifting a finger, simply by selling his assets and putting his money in savings banks. If his actual income is only $15,000 in a given year, the economist's analysis implies that he is paying $10,000 for the privilege of working his farm. From an accounting viewpoint, one might want to say that his "asset income" is $25,000 and that his "labor income" was -$10,000, bringing his total to the observed value of $15,000. Alternatively, one might want to say that if he works 2500 hours a year and if the minimum wage for farm labor in his area is $2.00 an hour, his labor income is at least $5000 and his asset income is/no more than $10,000. The possibilities are clearly endless. In the PSID, for example, SRC requires that labor incomes be positive. If a self-employed man loses money, the PSID assumes he has no labor income and a capital loss. Rather than trying to defend such arbitrary decisions, we have tried to use the simpler Census Bureau definition of earnings in all surveys.[11]

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[11] See Chapter 16 and Appendix D for discussion of the difficulties that arose when we tried to impose Census definitions on SRC's PSID data.

The choice of accounting period can also pose problems. Most economists prefer to treat how many hours a man works and how much he earns per hour as separate questions. Hours of work depend partly on one's potential hourly wage, partly on one's other income (i.e. income from assets, transfers, spouses), partly on economic need (number of dependents, the life style to which one aspires), and partly on unpredictable personal idiosyncracies ("the subjective value of leisure"). If individuals could always find work that utilized their talents for whatever number of hours they wanted to work, one could view all variations in hours worked as manifestations of personal preference. In fact, this is not the case. Most mature workers want a "steady" job. This means they prefer to be employed 52 weeks each year (including vacations). Most also want to work roughly 40 hours per week. They do not want to be laid off, look for a new job, make new friends, or learn new habits. But many employers do not offer steady employment of this kind. Those who do offer it can usually get workers with any specific set of characteristics cheaper than those who do not offer it. This means that an investigator who looks only at hourly earnings can get a misleading picture of the relative attractiveness of different jobs and of the relative bargaining power of workers holding these jobs. A private construction worker's hourly earnings are likely to be quite high, for example, but this is partly to compensate him for chronic temporary unemployment. A federal employee's hourly earnings may be more modest, but this is offset by the fact that he is less likely to be laid off or put on short time. Annual earnings probably give a better picture of the relative desirability of these two sorts of work than hourly earnings.

Whatever the theoretical merits of hourly vs annual earnings, practical considerations forced us to concentrate on annual earnings. All our surveys except Talent provided data on annual earnings or income. Only the Veterans and Talent surveys collected data on current weekly or hourly earnings. The PA, NLS, PSID, and Census provide data on weeks worked during the previous year. But these data are grouped in the PSID and Census, so that all respondents who worked between 1 and 13 weeks are coded in the same way, all those who worked 14 to 26 weeks are coded in the same way, and so forth. This means that when one divides annual earnings for the previous year by estimated weeks worked to get average weekly earnings, the quotient contains a certain amount of random error. These problems become even more serious when one tries to estimate hourly earnings. Census surveys typically ask how many hours the respondent worked during the week prior to the survey. Some investigators use responses for the previous week as a proxy for average weekly hours during the previous year. But hours of work vary considerably from week to week. This makes estimated hourly earnings even less reliable than estimated weekly earnings. We therefore decided to stress annual earnings wherever possible. The Appendices, however, introduce weeks worked as an independent variable where it is available, so that the reader can see how much of the variation in annual earnings depends on hours of work.[12/]

[12/] The reader interested in the determinants of weekly earnings can extract a considerable amount of evidence from our equations. Let Annual Earnings = A, Weekly Earnings = W, and Weeks Worked = S. Then $A = WS$, and $LnA = LnW + LnS$.
Thus
(1) $LnW = LnA - LnS$
and
(2) $S^2_{LnW} = S^2_{LnA} + S^2_{LnS} - 2S_{LnW}S_{LnS}r_{LnA,\ LnS}$

We present equations in the form
(3) $LnA = B_1 X_1 + B_2 X_2 + \ldots . B_s LnS + e_A$

When the X's include education and occupational status, $B_s = 1.00$ in all samples but the NLS. Subtracting LnS from both sides thus yields
(4) $LnW = B_1 X_1 + B_2 X_2 + \ldots .. e_A$

The B's in equation 3 thus provide unbiased estimates of the B's in an equation with weekly earnings as a dependent variable (i.e. equation 4)

Our third problem was what to do when either the respondent or the survey organization "rounded" earnings reports. Respondents have a strong tendency to round their estimates of their earnings to some convenient value./There is not much we can do about this. Partly because of this tendency, many survey organizations ask respondents to report their earnings in relatively broad categories. Some survey organizations argue that this reduces the nonresponse problem, though SRC's experience does not seem to support this view. Even when the original data are reported exactly, some organizations code these values into broad categories. This is especially common at the top, since there seems to be a widespread notion that reporting exact earnings over $25,000 or $50,000 might somehow violate the respondent's anonymity. Whatever the rationale, the result is a mess. Grouped data can yield biased estimates of both the effects of worker characteristics on earnings and overall inequality in earnings. A substantial fraction of the variance in earnings is variance among men earning more than $25,000 a year. A substantial fraction of the variance in the log of earnings is among men earning less than $1000 per year. Grouping usually eliminates such variance. Grouping therefore reduces measured inequality, at least if one chooses the correct mean for each group. But if one makes even a small error in selecting the category mean, one can end up overestimating the variance instead of underestimating it. If variation at the extremes depended on the same factors as variation in

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12/ continued:
so long as $B_s = 1.00$. The variance of e_A is also the same for equations 3 and 4, so R^2 in equation 4 is:

(5) $R^2 = 1 - S^2_{eA}/S^2_{LnW}$

the middle of the distribution, grouping would also reduce the correlation of earnings with other worker characteristics. But the characteristics measured in our surveys do not have as much effect at the extremes of the earning distribution as in the middle. As a result, grouping usually raises the observed correlation between earnings and other traits instead of lowering it. The combined effect of grouping on the variance and the correlations determines its effect on unstandardized regression coefficients. Experiments with OCG-II showed that grouped data raised almost all correlations by about a tenth, lowered the standard deviation of Ln Earnings by about a tenth, and thus left the unstandardized regression coefficients almost unchanged. McClelland obtained less uniform results with Census and PSID (see Chapter 16).

Except when comparability was our sole objective, we retained as much detail as possible in earnings. We would strongly urge other survey organizations and researchers to do the same. Grouping can only exacerbate the difficulties of getting meaningful results. The fact that grouped data can be coded in slightly less space on a computer tape is a ridiculous reason for eliminating information that is vitally important to many analyses. The argument that the data are error-prone and hence only approximate is equally fallacious.

A fourth difficulty in our work was that our data tape for the OCG included only information on income, not earnings. But 25-64 year old men typically earn about 95 percent of their reported income, so analyzing income rather than earnings has little impact on the results (see Chapter 16).

(b) Scaling Earnings

Having measured earnings (or income), one must decide how to scale the responses. Sociologists seldom study the determinants of earnings, but when they do, they usually use untransformed earnings as their dependent variable. We denote this measure as Earnings. Economists usually prefer the natural logarithm of earnings as a dependent variable. We denote this as LnEarnings.

We will emphasize results using LnEarnings more than results using Earnings. The most important advantage of LnEarnings over Earnings is that regressions using LnEarnings as a dependent variable are scale-invariant. If one multiplies everyone's earnings by a constant, this will simply add a constant to the logarithm of their earnings. Adding a constant to the dependent variable increases the intercept of the regression equation, but it does not affect the regression coefficients, their standard errors, or the variance of the residuals. This facilitates comparisons between years. If inflation or society-wide gains in productivity have increased earnings at all levels by the same percentage, the LnEarnings regressions will look alike. The Earnings regressions will look different.

A second advantage of LnEarnings is that if the regression coefficients of the independent variables are small, they can be interpreted as percentage effects. Suppose, for example, that Y is Earnings, X is IQ, K is a constant, and LnY = 0.01X + K. This implies that a one point increase in IQ raises the natural logarithm of earnings by 0.01. Increasing the natural logarithm of earnings by 0.01 is equivalent to multiplying actual earnings by $e^{.01} = 1.01005$. Increasing the natural log by 0.01 is thus equivalent to increasing dollar earnings by 1.01005 - 1 = 1.005 percent. As the example illustrates, the regression coefficient is not exactly equal to the percentage effect.

It always underestimates the percentage effect to some extent. The size of the bias increases as the coefficient increases. A coefficient of 0.05, for example, implies that a unit increase in the independent variable is associated with a 5.127 percent increase in earnings. A coefficient of 0.10 implies an effect of 10.52 percent. A coefficient of 0.20 implies an effect of 22.14 percent. A coefficient of 0.693 implies that each unit change in the independent variable is associated with a 100 percent increase in earnings.

Economists sometimes argue for the log of earnings on still another ground, namely that the subjective value of earnings (i.e. their "utility") is more likely to be a linear function of LnEarnings than of Earnings. Instead of assuming that a $500 raise is equally valuable to everyone, for example, they assume that a $500 raise is worth twice as much to a man with $5,000 as to a man with $10,000. But while a $500 increase is almost certainly worth more to a man with $5,000 than to a man with $10,000, it is not obvious that it is worth twice as much. Rainwater's data on popular assessments of hypothetical individuals' overall standing suggest, for example, that a $500 income increase is judged more valuable to a man with $5,000 than to a man with $10,000, but that it is judged less than twice as valuable. The same holds when one looks at the relationship between family income and personal happiness (Bradburn, 1968). All these sources of evidence suggest that one should use a power transformation of earnings to estimate their utility (Atkinson, 1972). Schwartz (1975) has assembled data suggesting that $\text{Earnings}^{1/3}$ is an optimal transformation. This transformation yields results intermediate between those obtained using Earnings and LnEarnings. It implies, for example, that if we measure well-being in some arbitrary metric(W), a $500 income increase raises the well-being of a man with \$5000 by $\$5500^{1/3}W - \$5000^{1/3}W = 0.55W$. For a man with \$10,000, a \$500 income increase raises well-being by $\$10,500^{1/3}W - \$10,000^{1/3}W = 0.35W$. It follows that \$500 is worth 1.57 times as much to a man with \$5000 as to a man with \$10,000. Rainwater's data on the relationship between income and estimated well-being are more nearly consistent with this view than with the idea that well-being is a linear function of either Earnings or LnEarnings.

The distribution of $\text{Earnings}^{1/3}$ is also closer to normal than the distribution of Earnings or LnEarnings. Since a worker's exact position in the tails of the distribution is not closely related to his other characteristics, and since normalizing the distribution reduces the relative importance of variance in the tails, normalizing tends to increase R^2. In addition, normalizing the distribution makes the estimated standard errors more accurate.

These considerations suggest that sociological analyses which treat Earnings as a measure of socio-economic status or of overall well-being should probably rely on $\text{Earnings}^{1/3}$ rather than Earnings or LnEarnings. This is particularly true if one is primarily concerned with standardized coefficients, as sociologists typically are. $\text{Earnings}^{1/3}$ does, however, have several serious drawbacks. First, it is unfamiliar. Second, the unstandardized regression coefficients from equations that utilize $\text{Earnings}^{1/3}$ as a dependent variable have no obvious interpretation. Third, unstandardized results using $\text{Earnings}^{1/3}$ are not scale invariant.[13] This makes it almost impossible to interpret differences between samples with different means. For those who are concerned with unstandardized rather than standardized results, then, either Earnings or Ln Earnings remains superior to Earnings$^{1/3}$.

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[13] **Schwartz (1975) argues that this may be an advantage.**

In light of all this, we decided to conduct parallel analyses using Earnings, LnEarnings, and Earnings$^{1/3}$. These analyses are presented in the Appendices. In the text, however, we stress LnEarnings.

3. The Relative Importance of Occupational Status and Earnings

Ideally, we would have liked to subsume all aspects of economic success into a single composite index. To do this, we would have had to get each respondent to assign a cash value to all the non-monetary costs and benefits of his job. This amount might be either positive or negative. Adding it to his cash wages would then give us his "true" earnings. One way to do this would be to ask people how large an annuity it would take to persuade them to quit working and never return to the labor market. Those who felt that their current or prospective employment was attractive for either monetary or non-monetary reasons would demand large annuities. Those who did not place much value on the jobs to which they expected to have access would settle for small annuities. The size of the annuity would be the net subjective value of a man's economic opportunities. Unfortunately, we have no data of this kind.

We do, however, have data that allow us to estimate the monetary value the public places on one non-monetary aspect of a job, namely occupational status. Rainwater asked his Boston respondents to provide magnitude estimates of the "general standing" of hypothetical individuals with various combinations of education, occupation, and income. A respondent's rating (W) is thus a function of the education (U), occupation (X), and income (Y) of that profile. When Rainwater regressed a profile's general standing on its education, occupational status, and income, the standardized regression equation was:

$$W = 0.2U + 0.2X + 0.6Y$$

Rainwater deliberately used more profiles with very high and very low incomes than with intermediate incomes. The standard deviation of Ln Income in his sample of profiles was thus 1.00, whereas the actual standard deviation of Ln income is about 0.70 for families with a male head aged 25-64. The standard deviation of Duncan scores was 24.9, which is almost identical to the population value for men 25-64. The equation thus implies that a 24.9 point increase in Duncan score would boost a profile's overall standing by a third as much as a 1.00 increase in LnIncome. A one point increase in Duncan score is thus equivalent of an increase of $1.00/(3)\ (24.9) = 0.013$ in LnIncome. Men concerned with maximizing their "general standing" in the eyes of the world should therefore be willing to take a 1.3 percent income cut in order to raise their Duncan score by 1 point. Conversely, they should be equally willing to take a 1 point reduction in Duncan score to raise their income by 1.3 percent.

The reader should obviously treat this estimate quite cautiously. It is not clear that "general standing" is synonymous with subjective "utility." So far as I know, nobody has asked respondents to rate the overall desirability of various outcomes from their own personal viewpoint. In addition, as Rainwater himself has noted, raters asked to assign a numerical estimate of general standing to a hypothetical profile may place more weight on income than on occupation simply because income is given in numerical form. In ranking real people, raters may take account of a much wider range of cues. These cues may depend more on occupation than income. Hodge (1970) has shown, for example, that a number of attitudes depend more on occupational status than on income. **If people use such attitudes as clues to one another's status, occupation could be more important than the public thinks it is. In the same vein, Sewell and Hauser (1975) found that a Wisconsin man's IQ score, educational attainment, and early occupational status depended more on his father's occupational status than on his parents' income. The sons' income, in contrast, depended more on his parents' income than on his father's occupational status.**[14/] **If the personal traits that maximize a child's life chances are similar to those that determine parents' social standing in their neighbors' eyes, occupational status is probably more important than Rainwater's data imply.**

It would also be useful to obtain data on the subjective importance workers place on occupation vs. earnings in their own lives.[15/] **It is**

[14/] Both father's occupation and parental income were taken from Wisconsin tax returns. Parental income was defined as the parents' mean income for the four years immediately after the son completed high school. Given the casual way in which tax forms ascertain occupation and the precision with which they try to ascertain income, the measure of father's occupation may be less reliable than parental income. For additional evidence on the relative importance of these two traits for sons' life chances see Bielby, Hauser, and Featherman (1976).

[15/] Richard Coleman's analyses of qualitative data collected in **several surveys** suggest that respondents emphasize occupational position more in explaining why other individuals rank lower than themselves than in exsignificant in this regard that NLS 45-59 year old men's statements about their overall satisfaction with their jobs depend more on their Duncan score than on their hourly wage (Quinn and Baldi de Mandilovitch, 1975). In our sample of 25-34 year old veterans, in contrast, job satisfaction correlates better with annual earnings (and still better with LnEarnings) than with occupational status.[16/] Even among veterans, however, earnings are nothing like three times as important as occupational status.

In light of all this, I am inclined to treat Rainwater's data as providing a minimum estimate of the price people are willing to pay for additional status. I would not be surprised if the average male were to prefer a 10 point advantage on the Duncan scale to a 15 or 20 percent income advantage. But a 10 point increase in a man's Duncan score would have to be worth as much as a 30 percent increase in earnings for occupational status to explain as much of the variance in perceived economic success as earnings explain. Rainwater's results suggest that relatively few people value occupational status this highly. It follows that desparities in earnings contribute more than disparities in occupational status to overall economic inequality.

15/ (continued) plaining why other individuals rank higher than themselves (Coleman and Rainwater, forthcoming). He says that respondents placed almost exclusive emphasis on income in explaining why another individual ranked higher than themselves. This suggests that low status raters should place more emphasis on income than high status raters, since low status raters would mostly be rating profiles that ranked higher than their own, while high status raters would mostly be rating profiles that ranked lower than their own.

[16/] Many of the other data sets reviewed by Quinn and Baldi de Mandilovitch presumably provide evidence on the relative importance of status and earnings for various aspects of job satisfaction. So far as I know, no one has reviewed the evidence on this issue systematically.

Chapter 12

Measuring the Effects of Worker Characteristics

by Christopher Jencks

This chapter explains how we measured four sorts of worker characteristics: educational attainment, labor force experience, cognitive skills, and family background. (Mueser describes our measures of personality traits in Chapter 5.) I also describe the statistical procedures we used to capture the non-linear and non-additive effects of these characteristics.

1. Respondent's Education

(a) Measurement

Ideally, we would like to measure three educational "inputs": the number of years the respondent spent in school, the subjects he studied, and the characteristics of the institutions he attended. We would also like to measure three "outputs": the highest grade he completed, the diplomas and degrees he received, and what he learned.

Our surveys provide little data on inputs. None measures the amount of time an individual spent in school, as distinct from the highest grade he completed. Since men repeat a year more often than they skip one, substituting highest grade completed for years in attendance leads to an underestimate of the "costs" of education and an overestimate of the likely rate of economic return to "investment" in education.

The Veterans and Talent surveys provide some data on the subjects respondents studied, since they asked respondents what high school curriculum they were enrolled in. The Talent survey also asked respondents their college major. Our analyses of both surveys showed that high school curriculum had significant effects on highest grade completed but that it had no independent effect on economic success after controlling highest grade completed. We therefore ignored high school curriculum in our analyses. We have not analyzed the effects of studying one subject rather than another in college or graduate school, since our Talent sample was not large enough to yield stable estimates for any but the most broadly defined fields.

PA, PSID, and Talent asked what college the respondent had attended. The PA sample provides no data on initial ability. It shows that men 25-64 who attended selective colleges had similar occupations but earned about 25 percent more than men from similar backgrounds who attended unselective colleges. The PSID data on college quality became available too late for us to include them in our analyses. Nor did we analyze the Talent data on college quality.

Our measures of educational output are only slightly more satisfactory than our measures of input. Census Bureau surveys routinely measure the highest grade the respondent completed. They do not ordinarily ask about the respondent's degrees, if any. At the graduate level, the Census lumps together all respondents with "six or more years of college." SRC, in contrast, lumps together men who finished 0 to 5, 6 to 8, and 9 to 11 years of school. At the college level, it does not measure years of education but instead distinguishes respondents with three kinds of credentials: those with some college but without BA's, those with BA's but no graduate degrees, and those with graduate degrees.

We defined "years of education" as the highest grade an individual had completed. We also refer to this as "educational attainment." We did not include vocational training in non-degree-granting institutions in our measure of "years of education." We estimated the mean educational attainment of men in each of SRC's educational categories and assigned this mean to everyone in a given category. Such grouping slightly reduces both the standard deviation of education and its correlation with economic success. It should leave the unstandardized regression coefficient of education essentially unbiased. But because the SRC education categories are based not only on the highest grade completed but on earning a degree, they systematically overstate the effect of school attendence per se. Those who attend graduate school but get no degree, for example, are ignored in calculating returns to graduate education. This tends to inflate both the correlation of education with economic success and the unstandardized regression coefficient of education. This bias roughly offsets the bias due to grouping. Thus when SRC collected data on highest grade completed in 1975 (too late for us to use it in our principal analyses), the correlation of this measure with the usual SRC measure was 0.976. The two measures' correlations with occupational status and earnings never differed by more than 0.01, and the direction of the difference varied from year to year.

(b) Scaling

Education does not constitute an interval scale in quite the same sense that, say, money does. A year of high school provides different experiences from a year of college. And while the fourth year of high school or college may be much the same as the third in educational terms, the fourth year usually leads to a diploma. These diplomas may have independent effects on economic success, over and above the effects of the education they symbolize.

One way to deal with such problems is to calculate the mean level of economic success for men at each educational level. One can accomplish this in a regression framework by regressing economic success on a series of dichotomous variables that represent completion of successive years of education. Thus if schooling is coded in years from 0 to 18, there will be 18 dummy variables. Their coefficients will represent the effect of completing a given year of school or college. R^2 will represent the percentage of the total variance in economic success explained by education, i.e. the percentage that falls between educational groups rather than within them. Equation 1 in Table 12.1 shows the value of R^2 (but not the coefficients) for such equations using the Census 1/1000 sample.

Equations of this kind have two drawbacks. First, since there are not many people in certain educational ategories, the means of these categories have large sampling errors. Second, even when the samples are large enough to generate regression coefficients with small standard errors, such equations produce more detailed information than most people want. Few readers want to ponder the reasons why those who finish 9th grade earn $500 more than those who finish 8th grade, while those who finish 10th grade earn only $270 more than those who finish 9th grade, even if the difference is statistically significant and holds up after controlling background and ability. Most people want to know whether certain broad classes of education have different effects from other classes of education. They may also want to know whether the last year of high school or college appears to have larger effects on earnings than prior years.

There is no good a priori basis for deciding that certain classes of education should be distinguished and others should be lumped together, so we conducted a variety of experiments. First, we identified two points at which "diploma effects" might be important: high school graduation and college graduation. We represented these two points with dummy variables. High School Graduation has a value of 1 if the respondent finished high school, 0 if he did not. College Graduation has a value of 1 if the respondent finished four or more years of college, 0 if he did not. We also identified four types of education: elementary education, secondary education, college education, and graduate education. We represented these four types of education with one comprehensive variable (Years of Education) and three "splines" (Years of Elementary School, Years of College, and Years of Graduate School). These variables measure what their names imply: the number of years of school an individual has beyond some

Table 12.1

Regressions of Occupation and Earnings on Education Using Various Specifications for Civilian, Non-Student, Non-Institutional Males Aged 25-64 with Positive Earnings in 1969: Census 1/1000 Complete Data Sample (N = 25,697).

Unstandardized Regression Coefficients of Independent Variables (With Standard Errors in Parentheses)

Dependent Variable		Years of Education	Years of Elementary School	Years of College	Years of Graduate School	High School Graduation	College Graduation	Years of Higher Education	R^2
Occupation	(1)	Dummies							.4244
	(2)	2.78 (0.18)	-1.59 (.26)	4.12 (.27)	[.00] (.28)	3.80 (.58)	.53 (.82)		.4236
	(3)	2.93 (.05)					4.01 (.77)	2.47 (.16)	.4111
	(4)	4.34 (.03)							.3853
Earnings	(1)	Dummies							.1536
	(2)	489 (65)	[-88] (94)	459 (98)	624 (101)	[333] (210)	758 (300)		.1527
	(3)	503 (20)					[543] (279)	550 (59)	.1523
	(4)	790 (12)							.1406
LnEarnings	(1)	Dummies							.1510
	(2)	.079 (.006)	[.001] (.009)	[-.010] (.009)	-.038 (.010)	[.012] [.020]	.085 (.029)		.1491
	(3)	.082 (.002)				.111 (.027)	-.026 (.006)		.1488
	(4)	.078 (.001)							.1481

Table 12.1 (Continued)

Dependent Variable		Years of Education	Years of Elementary School	Years of College	Years of Graduate School	High School Graduation	College Graduation	Years of Higher Education	R^2
Earnings$^{1/3}$	(1)	Dummies							.1804
	(2)	.467	[-.033]	[.069]	[-.048]	[.130]	.571		.1788
		(.038)	(.055)	(.057)	(.059)	(.123)	(.177)		
	(3)	.474					.670	[.014]	.1785
		(.012)					(.164)	(.135)	
	(4)	.530							.1767
		(.007)							

[Coefficients in brackets are less than twice their standard error.]

specified level. Years of College is thus 0 for everyone with 12 or fewer years of school, rising to a maximum of 4 for those with 16 or more years of school.

We then regressed each measure of economic success on all six education measures simultaneously. Equation 2 in Table 12.1 displays the results. Since equations of this type are not in general use, some explanation may be helpful. With all six variables in the regression equation, the coefficient of Years of Education is equal to the average effect of the years not specifically included elsewhere in the equation, i.e. Grades 9, 10, and 11. Completing 9th, 10th or 11th Grade thus raises a man's Duncan score by an average of 2.78 points. The coefficient of Years of Elementary Education represents the difference between the average effect of Grades 9 to 11 and the average effect of Grades 1 to 8. The average effect of Grades 1 to 8 on a man's Duncan score is thus the sum of the coefficient of Years of Education and the coefficient of Years of Elementary Education, i.e. 2.78 - 1.59 = 1.19 points. The standard error of the coefficient of Years of Elementary Education is the standard error of the difference between the effect of a year of elementary school and the effect of Grades 9 to 11. This difference is highly significant for Duncan scores. For Earnings, LnEarnings, and Earnings$^{1/3}$ the difference is insignificant. The coefficients of Years of College and Years of Graduate School are analogous to the coefficient of Years of Elementary School in that they also represent deviations from the secondary school "norm." The effect of years 1 to 3 of college is thus 2.78 + 4.12 = 6.90 points per year. The effect of a year of graduate school is 2.78 + 0.00 = 2.78 points. The coefficients of High School Graduation tells us that the last year of high school is worth 2.78 + 3.80 = 6.58 points, while the coefficient of College Graduation tells us that the last year of college is worth 2.78 + 4.12 + 0.53 = 7.43 points. If one assumes that the last year of high school or college increases "productivity" by the same amount as the previous three, and that selection on prior traits is also linear, the coefficients of High School Graduation and College Graduation represent "diploma effects" or "credentialism."

The six variable specification assumes that the effects of Grades 1 to 8 are all the same. It makes analogous assumptions regarding Grades 9 to 11, 13 to 15, and 17 to 19. Since this is not quite true, the six variable equation yields a slightly lower R^2 than the 18 variable equation. The principal reason is that the effects of elementary education are not monotonic. The handful of men reporting no education or only one year of education do slightly better economically than men reporting two or three years of education (see Table 9A, Appendix A). Since the coefficients of College Graduation and Years of Graduate School are insignificant in the Occupation equation, dropping these two variables would not lower R^2 significantly. This suggests that Years of Education, Years of Elementary Education, Years of College and High School Graduation predict occupational status about as well as 18 dummy variables.

When we turn to annual earnings, the picture is different. Once again, six education measures explain almost as much as 18, and two of these six are insignificant. But the two insignificant variables are no longer College Graduation and Years of Graduate School. Now they are High School Graduation and Years of Elementary School. For LnEarnings, Years of College is also insignificant, implying that we can get by with only Years of Education, College Graduation, and Years of Graduate School.

The correct way to deal with such findings is to choose the specification most appropriate to a given outcome and use that specification for all analyses involving that outcome. This was not what we did, however. For the sake of consistency, we decided to use Years of Education, Years of

Higher Education, and College Graduation to predict all dependent variables. We chose these three education measures not because they maximize R^2 but because they correspond to structural features of the educational and social system about which readers are already likely to have differentiated perceptions. Thus even when the coefficient of the "spline" (Years of Higher Education) is insignificant, that is a "finding." The same holds for the College Graduation dummy.

Equation 3 in Table 12.1 shows that this specification does not fit quite as well as the more elaborate models. But equation 4 shows that it fits better than the linear specifications used in most other research of this kind, especially when predicting occupational status.

Certain chapters in this report and a large body of other research investigate the determinants of educational attainment by regressing Years of Education on a respondent's other characteristics. This procedure implicitly assumes that all years of education are equally important. Our data suggest that matters are not this simple. Four years of college are worth twice as much as four years of high school if the yardstick is a man's eventual Duncan score. High school and college are worth almost the same amount if the yardstick is the percentage by which they increase earnings. Using our best estimate of "utility," i.e. $\text{Earnings}^{1/3}$, each year of education has roughly the same value, except that the last year of college is worth about twice as much as other years (see Table 12.1). These findings suggest that treating education as if each year were equally valuable makes as much **sense** as any single alternative assumption. Since treating years of education as an interval scale is certainly much easier than transforming education into a less intuitively comprehensible metric, we decided to proceed on this basis. Those whose primary concern is with occupational status rather than earnings should bear in mind, however, that higher education affects occupational status more than elementary or secondary education does, and that the determinants of years of higher education may not be exactly the same as the determinants of years of education as a whole.

2. Non-school Experience

We define "experience" as the number of years the respondent has been out of school **since age 14. If every respondent** entered first grade when he was six and advanced one grade per year, we could estimate experience from the equation:

(A) Experience = Age - (Education + 6)

But not all men enter first grade when they are six, and not all advance one grade per year. Furthermore, we do not have exact values for Education above 17 years in our Census surveys, and we do not have exact values at almost any level for SRC surveys.

Mincer (1974) tries to get around this difficulty by substituting the mean age at which men with a given amount of schooling last attended school **for** (Education + 6) in equation A. This would be reasonable if all education were full-time and continuous, but it is not. Mincer assumes, for example, that college graduates' mean age on entering the labor market is 25. This mean is presumably based on a skewed distribution. Most men probably get their BA at 22 or 23, but a few are much older. These older BA's are unlikely to have been in school continuously since the age of six. Most have interrupted their education to take part-time or full-time jobs. There is no obvious reason for ignoring such interludes. Certainly on-the-job training can and does occur on jobs taken prior to school completion.

We therefore adopted a slightly different approach. Like Mincer, we assumed that all respondents were enrolled continuously from their 6th through their 14th birthday, even if they did not complete 8th grade. This assumption is clearly inaccuarte for some older respondents, especially those raised on farms. But since additional non-scholastic experience has little impact on earnings among older respondents, misestimating these respondents' experience by a few years is of little consequence. Unlike Mincer, we assumed that after their 14th birthdays respondents lost only one year of non-school experience for each year of school they completed. In fact, more people repeat grades than skip them, so the typical respondent takes more than one year to complete the average grade. But many students are also likely to work part-time during the school year and full-time during the summer. When Talent asked its respondents to estimate the total number of years they had worked since 11th grade, and to report the highest grade they had completed, the results were almost perfectly consistent with our assumptions (see Appendix H, Table 2). This suggests that we can get a reasonable estimate of non-scholastic experience for young respondents using the following formula:

$$\text{Experience} = \text{Age} - 13 \text{ if Education} \leq 7$$
$$\text{Experience} = \text{Age} - (\text{Education} + 6) \text{ if Education} \geq 8$$

A 23 year old college graduate is thus presumed to have 23 - (16 + 6) = 1 year of non-school experience.

This formula makes non-school experience an almost perfect linear function of age and education. In the Census, for example, $R^2 = 0.996$ in a linear regression of experience on age and education. This means we cannot distinguish the joint effects of age and education from the effects of non-school experience. The only reason we use non-school experience rather than age in our analyses is that the effects of education and non-school experience on LnEarnings are almost additive, whereas the effects of education and age on LnEarnings are somewhat multiplicative. Additivity simplifies both data analysis and interpretation. It does not, however, constitute a very compelling argument for believing that the relationship between earnings and age/experience is *caused* by labor force participation rather than biological aging.

Our comprehensive definition of experience makes the relationship between experience and economic success difficult to interpret. Mincer (1974) and other human capital economists have argued, for example, that experience affects earnings because it is a proxy for "on-the-job training." Yet there is no direct evidence that years of non-school experience are a good proxy for the amount of on-the-job training men have had. Nor is there any direct evidence that on-the-job training accounts for an appreciable fraction of the observed association between experience and earnings.

Because of the way in which we constructed our experience measure, each extra year of schooling beyond 7th grade means a year less non-school experience for men of a given age. But since educated men live longer and have higher labor force participation rates than uneducated men, they end up spending as many years in the labor force as less educated men. An extra year of education therefore *delays* a man's working life rather than shortening it. As Mincer (1974) has argued, the net effect of an extra year of education on *lifetime* earnings is thus closer to the effect with experience controlled than to the effect without experience controlled (but see Psacharopoulos and Layard, 1976). The net effect of education on earnings *at a given age* is the difference between the benefits of a year of education and the cost of losing a year of experience for men of the specified age. One can estimate the net effect of education in two ways. One is to use an equation that controls age but not experience. Alternatively, one can control experience and then subtract the coefficient of experience from the coefficient of education to get the net benefit of an extra year of education. If the effects of experience are non-linear, as they usually are for samples with a wide age range, the net effect of an extra year of education will vary with age. Estimating the net effect of education is then a three step process. First, one determines how much non-school experience a respondent of a given age would have had if he had quit school during the year in question. Second, one estimates the marginal value of this year of experience. Third, one subtracts the marginal value of this year of experience from the coefficient of the relevant year of education. I will illustrate this calculation below, but first I must discuss the way in which we estimated the non-linear effects of experience.

(b) Scaling Experience

The effect of an extra year of experience on earnings diminishes as men accummulate more of it, reaching zero after 30 or so years out of school. The best way to get a clear picture of this relationship is to use a dummy variable for each level of experience. But just as with education, dummy variables yield more detailed information than most people actually want. If we could break experience into conceptually distinct categories, as we can education, we would use spline variables to summarize the non-linear effects of experience. But our surveys do not provide enough data on varieties of experience to justify this approach. We therefore treated non-school experience as a homogeneous good, measured on an interval scale (time). We then looked for a simple mathematical function that would reproduce the curvilinear effects of experience on earnings. We began with a general formula:

(1) $Y = B_0 + \bar{B}T + e$

where Y is a measure of economic success, B_0 is a constant, $\bar{B}$ is the average value of one's non-school experience, T is total non-school experience to date, and e is an error term embodying the effects of other traits. If $\overline{B}$ is independent of T, Y will increase by the same amount ($\overline{B}$) for each unit increase of T. This is the familiar linear model. But if $\overline{B}$ depends on T, Y will no longer be a linear function of T. The simplest alternative is to assume that $\overline{B}$ is itself a linear function of T, i.e.:

(2) $\overline{B} = B_1 + B_2T$

If the law of diminishing returns applies to experience, B_2 will be negative. Substituting from equation 2 into equation 1 one then has:

(3) $Y = B_0 + B_1T - B_2T^2$

Equation 3 implies that the relationship between experience and economic success is parabolic rather than linear. If the parabolic model is not significantly better than the linear model, B_2 will be insignificant and B_1 will not differ significantly from $\overline{B}$ in equation 1.

If one is interested in the marginal value of an extra year of experience rather than the average value of all years of experience, one takes the first derivative of equation 3, i.e.:

(4) $\frac{dY}{dT} = B_1 - 2B_2T$

In the Census, for example, the earnings equation for 25-64 year olds is

(5) $Y = 7.509 + .0422T - .00073T^2 + .0849U - .0159U' + .127U'' \pm e$

where Y is LnEarnings, T is Experience, U is Years of Education, U' is Years of Higher Education, U" is College Graduation, and e is an error term. The implied value of the 10th year of experience is the derivative when T = 10.5, i.e. 0.0422 - (2)(0.00073) (10.5) = 2.7 percent. The implied value of additional experience falls to zero when 0.0422 - (2)(.00073)T = 0, i.e. after 28.9 years of experience. To find the marginal value of the 14th year of education for a 29 year old, one first determines that a 29 year old with 13 years of schooling would be in his 10th year of experience, making the mean value of T equal to 10.5. Taking the first derivative, one finds that foregoing this year of experience reduces a man's earnings by 2.7 percent. But the 13th year of education raises LnEarnings by 0.0849 - 0.0159 = 0.0690. The net effect of the 13th year of school at age 29 is therefore to boost earnings by 0.069 - 0.027 = 4.2 percent. At 28.9 years of experience (i.e. age 48), an extra year of experience is worth nothing, so the 13th year of education boosts earnings by 6.9 percent.

There is, of course, no guarantee that the average value of an extra year of experience will be a linear function of the total amount of experience one has accumulated. The average value of experience could, for example, increase at a diminishing rate, but the rate of diminution could decline as T increased. Then:

(6) $\overline{B} = B_1 - B_2T + B_3T^2$

Substituting in equation 2 now yields:

(7) $Y = B_0 + B_1T - B_2T^2 + B_3T^3$

B_3 is highly significant in both the Census and OCG when predicting earnings. The Census equation for LnEarnings among 25-69 year olds is:

(8) $Y = 7.31925 + .07268T - .00214T^2 + .00002T^3 + .08643U - .01591U'$
$+ .12650U''$

Constraining B_3 to be zero, as equation 3 implicitly does, can therefore lead to misestimation of the effect of experience on earnings. This is particularly true for men with very little experience. Equation 5 implies that the first year of experience raises earnings by about 4.2 percent, whereas equation 7 implies an increase of 7.3 percent. Among 25-64 year olds, however, the two equations yield quite similar estimates. The typical 25 year old is in his seventh year of experience. Equation 5 implies that the seventh year of experience increases earnings by 0.0422 - (2)(7.5)(.00073) = 3.1 percent, whereas equation 7 implies that the seventh year is worth $9.07268 - (2)(7.5)(.00214) + (3)(7.5^2)(.00002) = 4.4$ percent. The differences for older men are even smaller. We therefore decided to use the quadratic form for most of our analyses.

Confronted with curvilinear relationships of this kind, it is tempting to transform experience so as to make its effects linear. Mincer, for example, obtained good results using a Gompertz transformation. He defines a new measure, T^*, such that $T^* = e^{-.15T}$ where e is 2.7183. The difficulty with this transformation--or any other--is that while it may produce linear effects for one outcome, it cannot produce linear effects in all outcomes. This reflects the fact that the curves for different outcomes are not parallel. The curve for occupational status rises far less rapidly than the curve for earnings during the early years of experience, and it keeps rising throughout men's working lives instead of declining after 30 years in the labor force. We therefore abandoned the quest for a transformation that would have linear effects on both occupational status and earnings. Instead, we stuck with the more flexible quadratic form.

Unfortunately, the linear and quadratic terms (i.e. T and T^2) tend to be highly correlated with one another. This means that when we enter both in the same equation, they have quite large standard errors. This makes it hard to tell when samples differ significantly. Such comparisons are doubly difficult if, as often happens, the quadratic term is insignificant in one of the two samples and is therefore omitted. In order to facilitate comparisons between samples we therefore created a set of orthogonal squared terms, i.e. terms that were uncorrelated with their linear counterparts.

The logic of this transformation is best illustrated by an imaginary example. Suppose that a sample includes the same number of men at each level of experience, and that T ranges from 1 to 39 years. The mean and median of T would then both be 20. The correlation of T and T^2 would also be very high. But if we define a new variable, $T^* = T - \overline{T}$, it will range from -19 to +19. **Since T^{*2} is equally large when $T^* = -19$ or $+19$, and the distribution of T is symmetric, the correlation between T^* and T^{*2}** is zero. Adding T^{*2} to the regression equation will therefore leave the coefficient of T^* unchanged. Furthermore, the increase in R^2 when one adds T^{*2} to an equation which includes only T^* is the square of the zero-order correlation between T^{*2} and the dependent variable.

In the real world, the distribution of T is not symmetrical. As a result, subtracting the mean from T will not make the quadratic term perfectly orthogonal to the linear term. There is, however, always some transformation of T that will accomplish this result. To find it, one estimates the regression equation:

(9) $T^2 = B_tT + e$

where e is the usual error term, uncorrelated with T by construction. Subtracting B_tT from both sides and setting $e = T^{*2}$ we have:

(10) $T^{*2} = T^2 - B_tT$

T^{*2} is now uncorrelated with T, so we estimate the regression

(11) $Y = B_4 + B_5T + B_6T^{*2} + e$

Substituting for T^{*2} from equation 10 we have

(12) $$\begin{aligned} Y &= B_4 + B_5T + B_6(T^2 - B_tT) \\ &= B_4 + (B_5 - B_6B_t)T + B_6T^2 \end{aligned}$$

One can use the coefficients in equations 10 and 12 to reconstruct the coefficients in equation 3. Comparing equations 12 and 3, we see that $B_0 = B_4$,

$B_1 = B_5 - B_6 B_t$, and $B_2 = B_6$. Thus the use of T^{*2} in place of T^2 changes only the linear coefficient of T. The coefficient of the squared term is unaffected. Furthermore, the linear coefficient of T controlling T^{*2} is equal to the coefficient of T with no controls. This means that one can easily compare equations that include T^{*2} to equations that omit it. Substituting T^{*2} for T^2 does, however, complicate the first derivative. Equation 4 showed that $dY/dT = B_1 + 2B_2T$. Substituting from equation 12, we have:

$$(13) \quad \frac{dY}{dT} = B_5 - B_6 B_t + 2B_6 T$$

This means that one must know B_t to calculate the marginal value of additional experience. The values of B_t for each sample appear in the Appendices. Note that in the Census sample we did not orthogonalize Experience^2.

3. Cognitive Skills

Psychologists have never been able to offer a satisfactory general definition of cognitive skills, so we will not attempt one here. Rather, we will follow their lead and define cognitive skills as "whatever cognitive tests measure." This is, of course, a meaningless definition unless one actually describes the tests. It may be meaningless even then. Nonetheless, it is the best we can do.

Our surveys used four tests, two of which were administered to adults with different amounts of formal education and two of which were administered to adolescents who had completed a specific amount of school.

A. The PSID Sentence Completion Test. SRC developed this test from the Lorge-Thorndike "intelligence" test. It only correlates 0.4 to 0.6 with other IQ tests. Each item is a sentence with a missing word. The interviewer reads the sentence and then asks the respondent which of five words belongs in the sentence.[1] The following is a fairly typical item:

"False facts are highly ________________ to the progress of science."

1.	Injurious	(69.2)
2.	Necessary	(11.0)
3.	Devoted	(2.2)
4.	Useful	(10.0)
5.	Instrumental	(5.6)
	No Answer, Don't Know	(2.0)

The percentage of 25-64 year old respondents giving each answer is shown in parentheses. Like most of the PSID items, this one does not have an unequivocally correct answer. But a respondent familiar with the culture from which this item emerged would almost certainly know that the kinds of people who conduct surveys probably believe that "injurious" is correct. Men who give the "correct" answer earn about 45 percent more than men who give one of the "incorrect" answers. Knowing what answer an interviewer wants thus appears to relate to other forms of success.

The best way to scale responses to such items would probably be to create a dummy for each possible response. One could then regress economic success on all these dummy variables (omitting NA's) to determine which specific responses predicted economic success and which did not. But we did not do this. Instead, we dichotomized responses to each item into the one SRC deemed "correct" and the four SRC deemed "incorrect." This simplification sacrifices information only if some allegedly "incorrect" responses are "less incorrect" than others. Having dichotomized each item, we found that "correct" responses to some items predicted economic success better than "correct" responses to others. But the ambiguous items predicted educational attainment and economic success at least as well as the unambiguous ones, and the differences between items were not statistically significant. We therefore decided to weight all 13 items equally and treat the total number of "correct" answers as a score. These scores have a mean of 10.0 and a standard deviation of 2.0 for men 25-64. The distribution is skewed, since no respondent can score above 13, while 5.8 percent score below 7. Differences within the bottom tail of the distribution generally have less impact on economic success than differences near or above the mean. This is what one would expect if the underlying distribution were normal. It is also what one would expect if low scorers included some who refused to answer all the questions out of irritation rather than ignorance. These non-linear effects are not apparent for all outcomes. We therefore decided against using a transformation to normalize the distribution of scores. Instead, we included Test Score^2 as well as Test Score in all regressions where Test Score^2 was significant. The Test Score^2 term usually has a positive sign, but it is not usually significant.

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[1] Although SRC tried to interview the male "head" wherever one was present, it had to settle for someone else (usually the wife) in one household out of seven in 1972. The spouse's test score is recorded on the data tape for those households. We eliminated these respondents from our analyses. Some other analysts have retained them. Wives' test scores presumably predict their husbands' economic success less accurately than the husband's own score would.

Mueser's analysis of the PSID data in Appendix D uses raw scores. Crouse's analyses in Chapter 3 convert these raw scores to the conventional "IQ" metric, i.e., a mean of 100 and a standard deviation of 15. This transformation has no substantive effect on the results, but it facilitates comparisons with other samples.

B. The Armed Forces Qualification Test (AFQT). This test is a descendent of the Army General Classification Test used during World War II and Army Alpha used during World War I. It is supposed to measure a respondent's capacity to carry out the tasks normally required of an enlisted man. It is a multiple choice, paper and pencil test, usually administered to large groups under less than ideal conditions. Prior to 1953 the AFQT consisted of 30 vocabulary items, 30 mathematical items, and 30 spatial relations items. These items were very similar to those used in other group tests that purport to measure "academic aptitude" or "intelligence." From 1953 on, the AFQT included 25 items from each of the three original categories, plus 25 items on the use of tools. The tool items appear to have lowered the correlation between AFQT score and educational attainment, while raising the correlation between AFQT score and efficiency ratings within the military.

We have AFQT scores for a sample of 25-34 year old veterans who were tested at the time of induction.

These men had completed different amounts of formal schooling when they took the AFQT, so their scores on the test are likely to depend in part on their educational attainment. Some veterans returned to school after military service. Most did not.

Our data tape did not include separate scores for the four AFQT subtests. Neither did it include the exact number of right or wrong answers. All we had were percentile scores, grouped in quite broad intervals. (The percentile scores are relative to men mobilized in 1944.) We rescaled these percentile ranks to a population mean of 100 and a standard deviation of 15, on the assumption that the underlying distribution was normal. When scaled in this way, the effects of test performance on economic success are essentially linear.

c. The Project Talent Academic Composite. Project Talent gave a two-day battery of tests to high school students in 1960. Our sample is restricted to males who were in the 11th grade at that time. The test battery covered a wide range of academic and non-academic subjects. Crouse describes the properties of these tests in detail in Chapter 4. He shows that the first principal component of the tests explains most of their variance, and that random error explains most of the rest. But he also shows that a broad measure of many different academic skills and information predicts eventual educational attainment and economic success slightly better than any single test does. Tests that do not correlate strongly with one another do not generally correlate with economic success either.

Most of our analyses of the Talent data rely on a measure that Talent labelled "Academic Composite." This measure includes tests of Vocabulary, Reading Comprehension, Mathematics, English, Abstract Reasoning, and Creativity. A respondent's composite score is the unequally weighted sum of his correct answers on these tests. The weighting scheme was based on a priori reasoning. The resulting scores have a roughly normal distribution. We rescaled them to a mean of 100 and a standard deviation of 15 for those who reached 11th grade. Crouse describes the test in more detail in Chapter 4 and in Appendix H.

d. The Terman and Otis"Intelligence" Tests. One or the other of these tests is available for all Kalamazoo 6th graders. They are group tests, primarily verbal rather than quantitative in character, and consist of multiple choice items. The two tests were not standardized to the same mean and variance. Olneck (1976) tried to correct this in the Kalamazoo sample by adjusting the means.

We do not know how strongly our different tests correlate with one another, so we cannot say to what extent they measure the same underlying traits. We do know, however, that other reliable tests of this kind typically correlate at the 0.65 to 0.85 level. Furthermore, Crouse shows in Chapter 4 that if one extracts the first principal component from a battery of heterogeneous tests, this principal component explains almost as much of the variance in economic success as the separate tests in a multiple regression. It therefore seems reasonable to proceed on the assumption that the different tests all correlate with economic success largely because they correlate with the same underlying trait or traits.

4. Father's Education

(a) Measurement

Father's Education is the highest grade of school or college the respondent said his father (or some designated substitute) had completed. Some surveys determined the exact number of years fathers completed. Others obtained categorical answers like, "Didn't finish elementary school," "Finished elementary school," "Entered high school, but didn't finish," and so forth. We coded categorical responses by estimating the mean number of years completed by men in each category and then assigning all respondents the estimated mean of their category. This procedure reduces the standard deviation of Father's Education and usually reduces its correlation with other traits, but the reduction is trivial.

Measuring father's education poses two problems that have not been dealt with adequately in the literature. First, one must decide how to handle men who were not living with their father when they were growing up. In OCG, for example, only 82.6 percent of all respondents said they lived with "both parents most of the time" when they were growing up. Another 3.7 percent said that they lived with their father alone. The remaining 13.7 percent said they were not living with their father "most of the time."

Most investigators see "father's education" primarily as a proxy for the cultural sophistication of the respondent's home. If the respondent was mostly living with a step-father, such investigators should determine the step-father's education. If the respondent mostly lived in a female-headed family, such investigators should determine this female's education. Census Bureau surveys such as OCG and OCG-II have been constructed on this "environmental" theory. They ask for the education of the head of the household in which the respondent was living at the age of 16. Men who were not living with their father most of the time are less likely to answer the question, but what we call "father's" education is the step-father's education or the mother's education for 12 percent of all OCG men aged 25-64.

A few investigators see father's education primarily as a proxy for father's genotype. If this view were correct, the correlation between father's education and the respondent's own characteristics would not depend on whether the respondent actually lived with his father when he was growing up. Conversely, a stepfather's education should have no partial correlation with the respondent's characteristics after controlling the mother's characteristics. The PSID was apparently constructed on these assumptions. It solicits information about the respondent's father. It does not ask whether the respondent's father was living at home. Neither does it solicit any information on step-fathers or other males who might have played a paternal role if the father was absent.

The best way to choose between the "genetic" and "environmental" interpretations of father's education would be to collect data on respondents who were not brought up in their father's home. One could then regress the respondent's characteristics on his father's and stepfather's characteristics simultaneously. Unfortunately, no data on this type are available.

OCG does, however, allow us to compare the effects of father's education on the characteristics of men who grew up with their fathers to the effects of stepfather's education on respondents who grew up with a stepfather. Table 12.2 shows the regressions of respondent's education on the education of the head of his household when he was 16, by type of household. Comparing the unstandardized regression coefficients, we can see that the effect of the head's education is weakest for respondents living with both their natural father and their natural mother. It is strongest for men living only with their mother. In households headed by some "other male," most of whom were presumably stepfathers, the effect of the head's education on the respondent's education is the same as for men living only with their natural fathers, and marginally larger than for men living with both their natural father and natural mother. None of these differences is statistically significant. I made analogous tables using other dependent variables, such as respondent's occupational status and respondent's earnings. But since father's education has very little direct effect on these outcomes after controlling respondent's education, these additional tables yield virtually the same results as Table 12.2.

These comparisons strongly support the "environmental" theory underlying the OCG approach to measuring "father's" education. The education of the household head affects the respondent in much the same way regardless of whether the two are genetically related. If this conclusion is correct, the "genetic" theory underlying SRC's approach to measuring father's education must be wrong. We cannot test this directly, however, since SRC does not distinguish fathers who were living at home from fathers who were not living at home most of the time when the respondent was growing up. For a conclusive test we need a large-scale survey that asks respondents about

TABLE 12.2

BIVARIATE REGRESSIONS OF RESPONDENT'S EDUCATION ON EDUCATION OF HOUSEHOLD HEAD WHEN RESPONDENT WAS 16 BY TYPE OF HOUSEHOLD FOR 10,895 OCG MEN AGED 25-64 IN 1962 WITH COMPLETE DATA[a/] AND NON-ZERO INCOMES in 1961.

TYPE OF HOUSEHOLD	REGRESSION COEFFICIENT (STANDARD ERROR)	R^2 (S.D. of RESIDUALS)	N (Response Rate)[b/]
Natural Father & Natural Mother	.39459 (.00841)	.1877 (3.248)	9537 (90.0)
If not living with father and mother at 16, head of household was:			
Father	.41583 (.03968)	.2279 (3.286)	374 (85.1)
Mother	.47494 (.03592)	.2382 (3.338)	561 (82.7)
Other Male	.41583 (.04690)	.2028 (3.559)	311 (81.6)
Other Female	.54514 (.07813)	.3669 (3.262)	86 (69.9)
No Answer	.34431 (.15022)	.1328 (3.317)	24 (32.9)
All	.40311 (.00787)	.1939 (3.278)	10,895 (88.2)

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a/ Respondent excluded if data missing on head's education, head's occupation, education, occupation, income, age, or household type.

b/ Response rate for father's education, based on all OCG men reporting specified household type (N=18,072), including those with missing data on items listed in note a.

both their father's education and their stepfather's education.

A second problem in measuring father's education is how to treat non-respondents. In OCG, for example, 11.8 percent of all men 25-64 with positive incomes failed to answer the question about their father's education. About a fifth of these non-respondents were not living with their father "most of the time" up to the age of 16. We assigned those men who failed to answer and who were not living with their father the mean, **and then included a dummy variable to capture the effects of not having a father at home.** This was an error in the case of OCG, since these men should have **reported on some** other head of their household. The procedure makes sense, however, for surveys that ask only about the natural father. It slightly reduces both the variance of father's education and its correlation with other outcomes. It should not affect the unstandardized regression coefficient, since the dummy variable for not having a father at home will capture any difference between these respondents' predicted and actual success.[2] Since SRC did not ask about family structure, we had to improvise various procedures for handling non-response in the SRC surveys. The appendices describe how we coded the relevant variables in each case. They also indicate how the coding procedures affected the correlation between father's education and other traits. The "pairwise" **correlations presented** in Table 2A of each appendix cover all men who actually reported their father's education. The correlations for the "complete data" samples include men who had complete data on all **items other than their father's characteristics and either (a) reported their father's education, or (b) failed to report/their father's education, were not living with their father at 16, and were therefore** assigned the mean. These differences are never large enough to be of substantive interest.

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[2] In Table 12.2, for example, the raw correlation between father's and son's education in OCG is 0.440. Appendix B covers the same sample except that it adds 609 men who said they were not living with their father and who failed to report their stepfather's characteristics. **We assigned these men the mean on father's education. The raw correlation in this sample falls to 0.428. The unstandardized regression coefficient is 0.403 in both samples.**

(b) Scaling

The effects of father's education are slightly non-linear in OCG and PA, with an extra year of school counting less at the bottom than at the top. In PSID, the pattern is reversed, although the deviations from linearity are never statistically significant. In retrospect, I would argue that we should have treated these non-linearities in the same way that we treated the non-linear effects of the respondent's own education, using spline variables that coincided with widely recognized differences in the institutional structure of education. Instead, we sought to capture the non-linear effects of father's education by including Father's Education^2 in our regressions whenever it was statistically significant. In order to maintain some degree of comparability between samples, we made Father's Education^2 orthogonal to Father's Education in our multivariate analyses (i.e. Tables 12-18 of the Appendices), though not in our bivariate analyses (i.e. Table 3 of each Appendix).[3/]

5. Father's Occupation

Most surveys ascertain father's occupation by asking the respondent what kind of work his father was doing when the respondent was 15 or 16. Classifying the responses poses the same theoretical and empirical problems

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[3/] Note that our procedure only maintains comparability between samples if the linear coefficients are similar in both samples. If the curvilinear coefficients are similar but the frequency distributions differ from sample to sample, the linear coefficients will differ. Our equations will reflect this difference, whereas an ordinary quadratic will not.

as classifying the respondent's own work. We tried to adopt the same solutions. The OCG, Veterans, NLS, NORC Brothers and Kalamazoo surveys classify father' occupations using the Census 3-digit code. The PSID classifies fathers using 9 broad Census categories. Talent used a multiple choice item that asked respondents to classify their fathers into one of 17 broad categories.[4/] The PA and Census did not ask about father's occupation.

Once again we had to decide how to deal with men who were not living with their father when they were growing up. I ran regressions of respondent's Education, Occupation, and Ln Earnings on the Duncan score of the head of his household when he was 16, using OCG data parallel to those in Table 12.2. Since none of the differences between men from households headed by the natural father, the natural mother, another male, or another female approached statistical significance and since they showed no consistent pattern, I have not reproduced the results. These findings suggest that father's occupation, like father's education, is a proxy for the cultural conditions in a man's home, not for parental genotype. Once again I was unable to test this conclusion directly, since I did not have data on both stepfathers and natural fathers of men who lived with stepfathers most of the time when they were growing up. Such data would be easy to collect and extremely useful.

The response rate among men who were not living with their natural father was somewhat lower than for other men. This was a particularly serious problem for men who grew up in households headed by their mother, since mothers were less likely to have had an occupation. Rather than eliminating

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[4/] For a discussion of the pitfalls of this procedure see Taylor (1976).

these respondents, we assigned them the mean. The dummy variable for not living with one's father should pick up any discrepancy between the level of economic success predicted on the basis of this assignment and the observed level.

One final problem is that some fathers change their occupations while their sons are growing up. Since a generation averages about 28 years, the typical son is reporting on his father's occupation when the father was about 44. The correlation between occupational status in 1970 and **a retrospective report of status in 1965 was 0.90 for Census men aged 40-49 in 1970.** The correlation between occupational status at age 44 and age 28, when the typical son was born, could be as low as 0.70. The correlation between a man's occupational status when his son was 15 and his mean occupational status between the time of his son's birth and the time when the son left home is unlikely to exceed 0.90. This suggests that if we are really interested in the father's mean occupational status, measuring his status at a single point in time is likely to introduce a certain amount of error. Survey researchers usually try to get around this difficulty by asking the respondent for his father's "usual" occupation "around the time" the respondent was 15 or 16. This is meant to eliminate short-term, temporary jobs, and it may well succeed in doing so. The correlation between responses to the usual survey question and the father's mean occupational status between the respondent's birth and his leaving home is, however, unknown.

6. Siblings

(a) Measurement

Asking a man how many brothers and sisters he had is fairly straightforward. There is room for ambiguity in the instructions (or lack of instructions) about whether to count half-siblings, step-siblings, foster-siblings, and siblings who died very young, but this is seldom a major problem. In addition, survey organizations differ in their treatment of very large families. Some truncate the distribution relatively low (e.g. "seven or more") while others obtain exact values.

(b) Scaling

The effects of additional siblings on education and economic success are consistently non-linear. A man with one sibling does better than anyone else. Men with no siblings do slightly worse than men with one, but better than others. Additional siblings consistently lower educational and economic attainment. But the marginal "cost" of the second sibling is higher than the marginal cost of, say, the ninth sibling.

It is not clear why only children should be handicapped relative to those with one sibling. Only children are more likely to come from female-headed households and to have low status fathers but this is not the whole story. They may be more likely to have unhealthy mothers, and to have had a difficult time in the womb. Or the absence of a sibling may be a handicap in its own right. In any event, the disadvantaged position of only children can only be captured if one uses a dummy variable to distinguish them from children raised in larger families. Unfortunately, we did not do this. Fortunately, only children constitute less than seven percent of all men 25-64, and they do almost as well as men with one sibling, so omitting the dummy is of little moment.[5] Nor is it clear why having more than one sibling should be a handicap. The most obvious explanation is that large families have to divide their economic and emotional resources among more competitors. If this were so, one would expect the effects of additional siblings to be a linear function of the respondent's share of the pie, however one defines "pie." If the number of siblings is S, each sibling's share of available resources is $1/(S+1) = S^*$. The effects of additional siblings should become linear when one regresses economic success on S^*. This is not actually the case. S predicts Education, Occupation, and Ln Income better than S^* in OCG. This is largely because S^* gives much worse predictions for only children. If one adds a dummy for only children (D), S^* and D together yield slightly more accurate predictions of both education and economic success than S and D. But even with D in the equation, the effects of S^* are not linear, since ${S^*}^2$ is highly significant when added to the equation. The notion that family size affects economic success by affecting resource shares is further weakened by the finding that additional siblings lower attainment significantly more for oldest children than for youngest children. (Middle children have intermediate coefficients.)While S^* and ${S^*}^2$ produce somewhat higher R^2's than S and S^2, the differences do not seem large enough to warrant the use of the unfamiliar and theoretically tenuous S^* transformation. If one wants an exact fit, one can get it by adding S^3 to the equation.[6/] We simply used Siblings and Siblings^2.

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[5] The difference in educational attainment between men with no siblings and men with one sibling is barely significant in OCG. Differences in occupational status and earnings are insignificant in all samples. But if one predicts education, occupation, or income using a linear or quadratic equation, only children have predicted values above men with one sibling, and the observed value is significantly below the regression line. The increment in R^2 after adding the dummy for older children never exceeds 0.002, however.

[6/] S^3 had a significant negative coefficient in OCG equaitons predicting education, occupation, and income. D, S, S^2, and S^3 yield values of $\bar{R}^2$ higher than 18 dummy variables. So do D, S^*, and ${S^*}^2$. Among OCG men 25-64 with complete data, $\bar{R}^2 = 0.1163$ with S and S^2 vs 0.1197 with 18 dummies. For occupation and income, both $\bar{R}^2$ and R^2 are smaller.

7. Race

(a) Measurement

Until recently, virtually all survey organizations told their interviewers to determine the respondent's race by visual inspection. The OCG, Veterans, PA, NLS, and PSID samples use this method. The 1974 NORC Brothers' survey instructed interviewers to ask the respondent if they had any doubt about his race. The Census and Talent samples relied largely on mailback questionnaires, which asked about race directly.

It is not clear what standards interviewers use to determine a person's race. Official procedures, such as those for birth certificates, have traditionally assumed the existence of a racial hierarchy running from white to yellow to red to black. Children of mixed parentage are then assigned to the race that ranks lower in the official pecking order. I suspect, but cannot prove, that interviewers reason in much the same way, classifying individuals as non-white if they have any clearly non-Caucasian features. If the respondent's physical characteristics are not easily classifiable, the interviewer probably assumes that the respondent is black if he lives in a black neighborhood, white if he lives in a white neighborhood, and so forth. If so, blacks living in predominantly white neighborhoods are more likely to be misclassified than blacks living in black neighborhoods. Visual classification may therefore overstate the degree of economic inequality between blacks and whites to some modest degree.

The usual alternative to visual classification is self-classification. This makes race a matter of personal identity, like ethnicity. A man is only "black" or "white" if he says he is. This seems perfectly reasonable for ambiguous cases, but when an individual looks clearly Caucasian or Negroid, his appearance may affect his economic success no matter what group he identifies with subjectively. In order to test this hypothesis we would need both an interviewer judgment based on appearance and a self-report. One might also want interviewers to treat appearance as a continuous rather than a categorical variable, i.e. "Very White," "More White than Other," "Very Black," "More Black than Other," and so forth. Needless to say, we do not have such data.

Regardless of how one defines race, it is theoretically possible for a child to belong to a different racial group than his parents. Indeed, so long as race is a categorical rather than continuous variable, children of mixed marriages must belong to a different group from at least one parent. None of our surveys inquired about the race of the respondents' parents, however, so we cannot distinguish the effects of parental race from the effects of the respondent's own race. We therefore had to assume that the respondents' parents were both of the same race as the respondent himself. This assumption is presumably correct in the great majority of cases. Unfortunately, this assumption also makes race both a family background characteristic and a characteristic of the respondent himself. We treat it primarily as a background characteristic, since that usage seems to coincide with what most people mean by "family background." But the reader should never lose sight of the fact that race differs from other background characteristics in that it is also a visible characteristic of the respondent.

We classified respondents as "white" and "non-white." In some tables we also distinguish between "blacks" and "other non-whites." In most cases, "others" are mainly Orientals and American Indians. In the PSID, however, Mueser classified "Spanish-American" respondents as "non-whites." This means there are more non-whites in the PSID than in other surveys. More important, it means that only 72 percent of PSID non-whites are black, compared to around 88 percent in other samples. One must therefore be more cautious in generalizing from non-whites to blacks in the PSID than in our other samples. In retrospect, I think it would have been wiser to distinguish blacks and non-blacks throughout our analyses, as Duncan and his colleagues have done.

(b) Scaling

Since we tried to scale most variables so that advantages had high values, we assigned whites a one, non-whites a zero. The unstandardized coefficient of "white" is therefore the difference between whites and non-whites. In large samples that include dummy variables for both White and Other, the coefficients represent these two groups' respective advantage over the omitted group, namely blacks.

8. Father Foreign Born

This is a dichotomous variable with a value of 1 if a man says his father was born in a foreign country, 0 if he says his father was born in the United States. The measure is available in the OCG, Census, PA, PSID, NLS, and Kalamazoo. It generally had a positive effect on economic success, but this effect was quite small and disappeared when we controlled current region of residence and size of city. As a result, we did not include it in most of our analyses of the Census and OCG samples. We did include it in analyses of the PA, PSID, NLS, and Kalamazoo analyses. The difference reflects the difficulty of getting large numbers of investigators to follow precisely comparable procedures, even when they work on the same project.

9. Father Not at Home

This dichotomous variable is supposed to measure whether the respondent grew up in the same household with his natural father. Since different surveys asked different questions about the structure of the household in which the respondent grew up, it does not have quite the same operational meaning in different surveys. SRC surveys do not ask the question at all, though Mueser constructed a proxy for the PSID. The reader interested in the unique effects of family structure should consult the relevant appendices to see how the variable was constructed for a particular survey before trying to interpret the coefficient.

10. Non-South Upbringing

This is a dichotomous variable based either on the state in which the respondent said he was born or on the state in which the respondent said he lived "most of the time" when growing up. The "non-South" includes all states north of the Mason-Dixon line and the Ohio River, and all states west of the Mississippi except Arkansas, Louisiana, Texas, and Oklahoma. It also includes foreign countries.

11. Non-farm Upbringing

This is a dichotomous variable based either on the respondent's report of whether he grew up on a farm or, if that was not available, on the respondent's report of his father's occupation. The two measures are not synonymous. Some respondents report that they grew up mainly on a farm even though their father's principal occupation around the time they were 16 was not in agriculture. This could happen in several ways. Some respondents presumably grew up mainly on a farm but moved off the farm by the time they were 16. Other respondents presumably lived on marginal farms that would not support the family, and had fathers who worked in some other job to make ends meet. This is quite common in marginal farm communities. Conversely, some respondents report that their father was a farmer or farm laborer when they were 16, but that they did not live on a farm most of the time when they were growing up.

Multiple regression shows that both growing up on a farm and having a father engaged in agriculture affect educational attainment and economic success. This holds even with the father's Duncan score controlled. The independent effects are not large, however. Since the two variables are highly correlated, we did not try to distinguish them in our principal analyses.

Chapter 13

The Reliability of Socio-economic Measures

by Christopher Jencks

Survey respondents do not always answer questions accurately. In some cases the distortions are systematic. In other cases they are essentially random. We have very little good evidence regarding systematic distortions. This chapter deals entirely with random errors.

The conventional method for describing the magnitude of random errors is to report a "reliability coefficient." This is the correlation between two **independent** estimates of the same underlying characteristic. By "independent" we mean that errors in the two estimates correlate with one another only insofar as they correlate with true values. If the errors in each estimate are uncorrelated with the true values, the total variance of each measure will be the sum of the true variance and the error variance. The reliability coefficient will then equal the ratio of the true variance to the total variance. But if respondents' reports are constrained to fall within some limits, i.e. if the measure has a "ceiling" or a "floor", errors will tend to be negatively correlated with true values. The measured variance will then be the sum of the true variance and the error variance, minus twice the covariance of the true values and the errors. If errors are correlated with true values in this fashion, the reliability coefficient no longer estimates the ratio of the true variance to the error variance. It does, however, still estimate the percentage of variance in the measured values explained by the true values. It therefore equals the square of the correlation between measured and true values.

The amount of error in reporting a given characteristic depends on the precise wording of the question and on the characteristics of the respondents. It may also depend on the characteristics of the interviewer and on the year of the survey. Generalizing about reporting error from one survey to another is therefore hazardous. This will be particularly true if differences between the two surveys have altered the variance of observed responses. Such observed differences can be due to differences in the true variance, the error variance, or the covariance of true values and errors. One seldom knows which is really involved.

If observed variances differ solely because the target populations differ, this usually implies that the true variances differ. The error variance may or may not differ. If the error variance and the correlation between errors and true values are the same in both surveys, one can use a reliability coefficient from one survey to estimate the coefficient for the other survey. The observed variances may, however, differ because of the way in which the questions are worded or coded. This is likely to be the case when one survey groups responses while another does not, or when the grouping procedures differ between surveys. Grouping usually reduces both the true variance and the error variance. If both the true variance and the error variance fall by the same percentage, and if the correlation between errors and true values is negligible, the reliability coefficient will be the same before and after grouping.

In what follows we will report both reliabilities and the estimated error variance. Our estimates of error variance assume that errors are uncorrelated with true values. Since this is unlikely, the error variances should be treated cautiously.

If errors are uncorrelated with true values, and if Var(E) is the error variance, Var(M) is the measured variance, and r* is the reliability coefficient,

$$\mathrm{Var}(E) = (1 - r^*)\,\mathrm{Var}(M)$$

1. Occupational Status

If different interviewers ask the same man to describe his work, he will not always give exactly the same answer. Even if he does give the same answer, different coders may assign him to different occupational groups. This means that there is a certain amount of random error in any single set of estimated Duncan scores.

The reliability of Duncan scores for men's current occupations has never been estimated very satisfactorily. The best data base appears to be that collected in connection with OCG-II. The March, 1973, CPS obtained occupational data from either the OCG respondent or (more often) his wife. CPS reinterviewed about 1000 of these respondents by telephone in the fall of 1973. Bielby, Hauser, and Featherman (1976) have analyzed these data for 578 white males aged 20-64. They report a correlation of 0.797 between Duncan scores assigned on the basis of the March job descriptions and scores assigned on the basis of the fall job descriptions. The "error" variance for the two surveys has a geometric mean of $(1-0.797)\ (24.81)\ (25.21) = 11.27^2$. This variance has at least three distinct sources:

1) Assignment of occupations to men for whom no data were available in March, 1973.
2) Real changes in status between March, 1973, and the following fall.
3) Actual errors in reporting men's occupations.

In order to estimate (3), we must first estimate (1) and (2).

(1) Bielby found that 0.5 percent of all OCG II reinterview respondents failed to report an occupation in the March interview. Another 1.7 percent failed to report their occupation the following fall. Deleting these men raised the correlation from 0.797 to 0.825 (Bielby, in correspondence).

(2) CPS did not ascertain how many OCG-II respondents had changed occupations between March and the following fall, but the January, 1973, CPS asked men to report their occupations in both January, 1973, and January, 1972. Since the two reports were simultaneous, men who had not changed jobs simply reported that fact. Among men aged 20-64 who had jobs at both times, 8.9 percent described the two jobs in such a way that Census coders felt they had changed their detailed occupation. I therefore infer that in the period between the two OCG-II interviews at least 8.9/2 = 4.5 percent of all respondents had changed occupations. This estimate may well be too low, however. Miller (1977) found almost twice as much apparent occupational movement in longitudinal data from NLS as in retrospective Census data over a five year interval. If the same holds for one year retrospective data, as many as 9 percent of all respondents may have changed jobs over a seven month period. Some of the apparent movement in NLS is, however, due to measurement error rather than real movement. I will therefore treat 4.5 percent as a reasonable value. Among Census respondents aged 25-64 in 1970 who had changed occupations during the previous five years, the correlation between Duncan scores in 1965 and 1970 was 0.591.

Assuming equal means and variances for job changers and non-changers, the observed correlation between spring and fall Duncan scores would be 0.825 = (0.045)(0.591)+(0.955)(r) where r is the correlation between spring and fall reports for men who did not change occupations and reported at both times. Solving, r = 0.836.

After comparing the correlations of the March CPS occupation report and the fall reinterview report to other variables, Bielby et al concluded that the March report had a reliability of 0.84 while the fall report had a reliability of only 0.76. This difference is partly due to the fact that the fall reports include more allocated values. If we were to eliminate men with allocated values, the difference in reliability would probably be no more than 0.05. Thus the estimated reliability of the March reports is probably around 0.86, (Note that two-thirds of the March reports came from spouses, compared to only a third of the fall reports. Wives evidently report their husband's occupation about as well as he does.)

If the ratio of error variance to true variance is the same within broad occupational groups as between such groups, the relaibility of occupational status estimates will be the same regardless of whether we use broad or detailed categories. Unfortunately, we do not have reliability data using broad occupational categories for the OCG-II reinterview sample. The Census Bureau has, however, cross-tabulated respondents' broad occupation groups as reported to the March, 1960, Census and the March, 1960, Current Population Survey. Siegel and Hodge assigned Duncan scores to the broad occupational groups, excluded non-respondents, and obtained a correlation of 0.861 between the two reports. It is not clear whether the Census or CPS reports is likely to be more reliable. In the absence of evidence, it seems best to assume that the two reports are equally reliable.

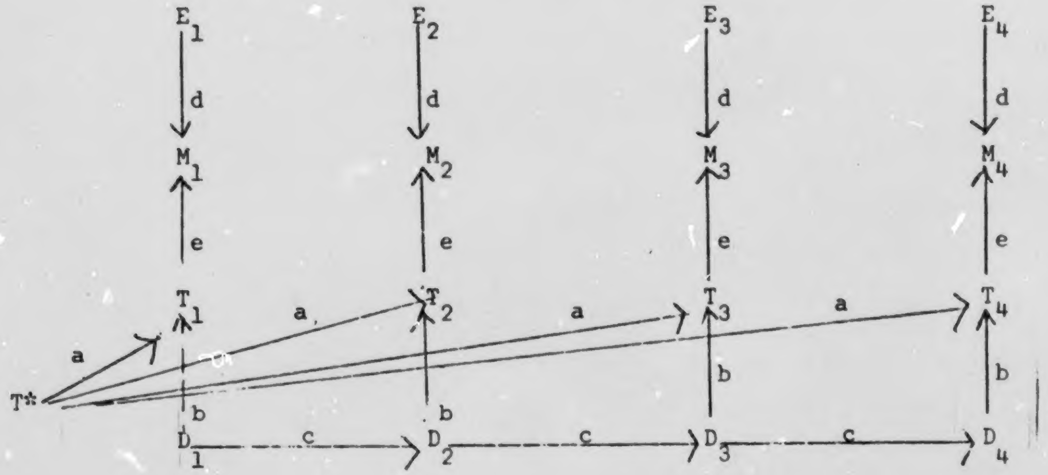
One can also estimate the reliability of occupational status scores from the PSID by looking at the correlations between status scores in sequential years. Let us suppose that a man's "true" score in a given year (T_i) is the sum of two components: a "latent long-term score" (T^*) and a transient deviation from the latent long-term score (D_i). Let us define the latent long-term score as the mean score of all men with a specified set of stable characteristics, measured and unmeasured. Let us suppose that a man's deviation from his true long-term score depends on temporary factors that are governed by Markov rules. This implies that the correlations between successive values of D_i decay at a geometric rate such that $r_{D1,D3} = r^2_{D1,D2}$ while $r_{D1,D4} = r^3_{D1,\ D2}$ and so forth. Now let us further assume that men's measured scores in a given year (M_i) are the sum of the true score and an error (E). Finally, assume that errors are uncorrelated over time.

Figure 1 summarizes these assumptions in a path diagram. The model has five unknowns. One needs four years of data to estimate these five unknowns, so the model has been drawn to cover four years. It could easily be extended to cover five or more years. Each additional year provides an additional equation but creates no additional unknowns. Additional years of data therefore allow us to test the validity of the assumptions used to construct the model. The model also assumes that the observed interannual correlations are stable over the period under study. This assumption should be tested before using the model.

Algebraic manipulation of the equations in Figure 1 shows that

FIGURE 1

POSSIBLE PATH MODEL FOR DETERMINANTS OF MEASURED ECONOMIC SUCCESS IN FOUR SEQUENTIAL YEARS


E1
E2
E3
E4
d
d
d
d
M1
M2
M3
M4
e
e
e
e
T1
T2
T3
T4
a
a
a
a
T*
b
b
b
b
D1
D2
D3
D4
c
c
c

Equations

(1) $1 = d^2 + e^2$

(2) $1 = a^2 + b^2$

(3) $r_{M1,M2} = r_{M2,M3} = r_{M3,M4} = (a^2 + b^2 c)(e^2)$

(4) $r_{M1,M3} = r_{M2,M4} = (a^2 + b^2 c^2)(e^2)$

(5) $r_{M1,M4} = (a^2 + b^2 c^3)(e^2)$

(6) $$c = \frac{r_{M1,M3} - r_{M1,M4}}{r_{M1,M2} - r_{M1,M3}}$$

(7) $$b^2 = \frac{r_{M1,M2} - r_{M1,M3}}{r_{M1,M2}(1-c^2) - r_{M1,M3}(1 - c)}$$

(8) $$e^2 = \frac{r_{M1,M2}}{1 - (1 - c)b^2} = r_{M1,M2} + \frac{(r_{M1,M2} - r_{M1,M3})^2}{r_{M1,M3} - r_{M1,M4}}$$

Since e is the correlation between the measured value and the true value in a given year, e^2 is the expected correlation between two independent measures in the same year, assuming the errors are uncorrelated. e^2 is therefore the reliability.

If we substitute the mean values of $r_{M1,M2}$, $r_{M1,M3}$, and $r_{M1,M4}$ from 8 years of the PSID data on 25-64year olds into this model, the average values are $a^2 = .838$, $b^2 = .162$, $c = .743$ and $e^2 = 0.952$. I have not used maximum likelihood procedures to refine these estimates, nor have I conducted formal tests for goodness of fit. But even if the fit is satisfactory, the standard error of e^2 is large. Taken at face value, my results imply that PSID occupational status has a reliability of 0.952, which is far higher than Census or CPS.

We can set a lower limit on the reliability of the PSID measure that has a much smaller sampling error.

The correlation between measured scores in sequential years is

$$(9) \quad r_{M1,\ M2} = e^2\ r_{T1,T2}$$

so

$$(10) \quad r_{T1,T2} = \frac{r_{M1,M2}}{e^2}$$

Likewise, the correlation over a two year interval is:

$$(11) \quad r_{M1,\ M3} = e^2\ r_{T1,T3}$$

so

$$(12) \quad r_{T1,T3} = \frac{r_{M1,M3}}{e^2}$$

On any of plausible assumptions, including those in this model, we can say that

$$(13) \quad r_{T1,T3} \geq r_{T1,T2} r_{T2,T3}$$

Substituting from equations 10 and 12 into equation 13 and multiplying through by $e^4/r_{M1,M3}$, we get:

$$(14) \quad e^2 \geq \frac{r_{M1,M2}\ r_{M2,M3}}{r_{M1,M3}}$$

Applying this equation to the PSID data we get $e^2 \geq$ 0.943. This value has a sampling error of about 0.02. The PSID data thus appear more reliable than than the Census data on broad occupational group.

2. Earnings

None of our surveys **reinterviewed the same respondents** about their earnings in a given year. The Census Bureau has, however, collected data on earnings in the same year using two different survey methods. And SRC has collected data in sequential years using the same method.

The Census Bureau publishes cross-tabulations of individual incomes as reported to the decennial Census and to the Current Population survey in the same year. Siegel and Hodge (1968) obtained a correlation of 0.82 between Census and CPS reports of 1959 income for men 14 and over. McClelland obtained a correlation of 0.78 between Census and CPS reports of 1969 income for men 14 and over with non-zero income.

In general, eliminating men with zero income tends to raise income correlations, so the actual difference between 1959 and 1969 is probably 0.05 or 0.06 rather than 0.04. Since these samples cover about 5000 men, the difference is highly significant.

One possible explanation is that both sets of data are grouped. Applying the 1970 Census income categories to the PSID lowered the interannual correlations by about 0.04. The magnitude of the difference depends, however, on the relationship of the income categories to the mean. These relationships changed between 1959 and 1969. The correlation of the grouped responses could therefore have changed even though the correlation using the raw data **did not**. This could account for most of the observed difference.

There is another puzzle, however. Mean income rose substantially between 1959 and 1969. The standard deviation rose proportionately. Thus if the absolute magnitude of respondents' errors/were proportional to their true income, changes in the mean would not affect the ratio of the error variance to the total variance. The reliability would therefore remain stable. But the cross-tabulation of Census and CPS responses shows that discrepancies between responses are not proportionate to the mean response. Men who report high incomes tend to have larger discrepancies than men who report low incomes, but the discrepancies do not increase as fast as the mean. If this pattern persisted over time, and if the ratio of the true standard deviation to the mean were stable over time, the ratio of the error variance to the true variance would fall. This would make recent income reports more reliable than older reports. Since no such improvement is apparent between 1960 and 1970, we must entertain the hypothesis that respondents at any given income level were less careful in 1970 than in 1960.

If Census and CPS reports were equally reliable, the correlation between the two would provide an estimate of their reliability. But Census reports are obtained largely from mailback survey filled out by "the householder," while CPS reports are obtained from interviews with any adult in a household who is at home when the interviewer calls. This implies that Census data come mainly from males, while CPS data come mostly from their wives. I would expect males to give more consistent estimates of their incomes than their wives give for them. I would therefore expect the Census reports to be somewhat more reliable than CPS reports. McClelland found, however, that the standard deviation of Census wage and salary reports was $6257 vs $5820 for the same individuals' CPS reports. Thus if Census reports are more accurate than CPS reports, CPS errors must have a stronger negative correlation with true values than Census errors. This could happen if wives who were uncertain about their husbands' incomes tended to report values near the mean. Alternatively, the CPS data may be more reliable, and the Census standard deviation may be inflated by random errors.

We can get some additional evidence on the reliability of CPS wage and salary reports from published cross-tabulations of husbands' and wives' total 1972 wage and salary income as reported to CPS and to the Internal Revenue Service. Reports to IRS are obviously not flawless, but since most wage and salary data is first reported by employers and then verified by the recipient, and since there are severe penalties for failure to report income, the data are probably quite good. In any event, it seems reasonable to hypothesize that CPS respondents treat their reports to the IRS as the standard they should approximate when talking to CPS interviewers. If they have misled IRS, either deliberately or accidentally, they will probably treat this deception as if it were the truth when reporting to CPS. IRS reports thus constitute "true scores" in the psychometric sense, since if one could average large numbers of CPS interviews of the same individual, the mean would converge on the IRS value.

McClelland obtained a correlation of 0.887 between CPS and IRS reports of total wage and salary incomes for 33,390 matched households, including some that reported no income from wages and salaries. Excluding households with no wage or salary income raised the correlation to 0.904. Taking logarithms then raised it to 0.922, suggesting that discrepancies between CPS and IRS are larger at the top. If CPS reports vary around the IRS value, the implied CPS reliability for couples with positive wage or salary income is $0.904^2 = 0.82$. The correlation between Census and CPS reports of wage and salary income for 5036 males with positive reported values in both Census and CPS is 0.84. The two figures are not strictly comparable, since one pertains to the sum of male and female wages while the other pertains to male wages alone. Nonetheless, the pattern is about what we would expect if IRS reports were completely accurate and Census reports were slightly more accurate than CPS reports. My reading of these data is thus that grouped CPS wage and salary reports had a reliability of about 0.82 in 1970, while grouped Census wage and salary reports had a reliability of about 0.86.[1] Applying equation 14 to the interannual correlations in the PSID suggests that the reliability of the raw reports is about 0.04 higher than the reliability of the grouped reports.

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[1] For an alternate reading of the same data that assumes random errors, see

The analyses in this volume are concerned with earnings from all sources, not just earnings from wages and salaries. Earnings from self-employment are not as reliably reported as earnings from wages and salaries. The 343 men who reported non-farm self-employment income to both the 1970 Census and the 1970 CPS, for example, reported values that correlated only 0.69. The value for farm income might be even lower. Fortunately, not many men have much income from self-employment. Thus the reliability of total earnings is probably only 0.01 or 0.02 less than the reliability of wage and salary income.

All these estimates are for men 14 and over. McClelland found that restricting the age range to 25-64 year olds did not alter the interannual correlations in PSID. This suggests that reliability is about the same for men 25-64 as for all men 14 and over.

After correcting the Census-CPS figures for the exclusion of self-employment income and for the effects of grouping, the implied reliabilities are about 0.84 for CPS earnings and 0.88 for Census earnings. The implied reliability of LnEarnings is about 0.03 higher than the implied reliability of Earnings, at least judging by the CPS-IRS match.

In order to estimate the PSID reliabilities, McClelland looked at correlations between sequential reports of annual earnings for PSID men aged 25-64 in 1970. Inserting PSID data in equation 14 suggests that the reliability of annual earnings was at least 0.89 in 1968, 0.88 in 1969, and 0.95 in 1970. The standard errors of these results are about 0.03. Their geometric mean is 0.91. For LnEarnings, the minimum estimate of reliability averages 0.86. The PSID pattern is thus the opposite of the Census-IRS pattern, where taking logs raised the estimated reliability. These correlations suggest that PSID earnings data may be slightly more reliable than CPS or Census earnings data, but that this is not true for LnEarnings. But since equation 14 yields a minimum rather than a point estimate of reliability, the PSID reports could be more reliable than Census or CPS reports for LnEarnings as well.

3. Education

The 1960 Census followed up 9000 respondents using the same procedures as the original Census. Bishop (1974) reports that the two reports of education correlated 0.915. The correlation was the same for the 7500 respondents contacted via face-to-face interview as for the 1500 respondents who returned mailback questionnaires. These data cover both males and females 25 and over. The error variance (assuming random errors) is 1.04^2 years.

In 1970 the Census matched about 20,000 individuals' reports of their educational attainment with their reports to the 1970 CPS. Bishop reports correlations of 0.887 for males and 0.875 for females. These numbers are not strictly comparable to those for 1960, since the true variance changed in the interval. If the error variance had been the same for 1970 Census reports as for 1960 Census reports, the implied reliability of the 1970 Census reports would have been about 0.90. If the error variance was stable from 1960 to 1970, then the implied 1970 CPS reliability would be about $0.88^2/0.90=0.86$. This implies that CPS reports are less reliable than Census reports. But the Census and CPS variances are virtually identical, and the 1960 Census reinterviews suggest that face-to-face interviews yield education reports exactly as reliable as a mailback questionnaire. An alternative explanation is that 1970 Census and CPS reports were equally accurate, but that neither was quite as accurate as the 1960 Census reports. The implied 1970 reliability for males 25 and over would then be

0.887. With random errors, the implied 1970 error variance would be $(1-0.887)(12.1) = 1.17^2$ for males and $(1-0.875)(9.4) = 1.08^2$ for females.

As a partial check on these estimates one can compare March, 1973, CPS reports on non-black males aged 20-64 to their reports in the OCG-II mailback survey in the fall of 1973. Hauser (in correspondence) reports a correlation 0.854 between the two sets of reports ($N\sim25{,}000$). The standard deviation of the CPS reports was 3.07 years. If we accept the evidence of the 1960 Census, OCG-II's mailback reports should be as accurate as the CPS interviews. This implies that the CPS interviews had an error variance of $(1-0.854)(3.07)^2 = 1.17^2$ years. This is identical to the 1970 Census-CPS estimate, even though the OCG-II data do not cover blacks or men over 65.

These estimates may be compared with Bielby et al.'s analysis of the OCG-II reinterview sample. The reinterview sample included 578 non-black males aged 20-64. These men not only reported their education in the March CPS (U_0) and the OCG-II mailback (U_1) but also in a telephone interview after the OCG-II mailback (U_2). Bielby et al. report that for this subsample $r_{01} = 0.801$, $r_{02} = 0.921$, and $r_{12} = 0.838$. The value of r_{01} for this subsample differs from the value for the full OCG sample (0.854) by 2.7 to 3.7 times its standard error, depending on the estimated efficiency of the reinterview sampling frame. Either the March CPS report or the OCG-II mailback report was considerably less accurate for the reinterview subsample than for the full OCG-II sample. After analyzing the correlations between the three education reports and the respondents' other traits, Bielby et al. concluded that the reliabilities for this subsample were 0.89 for the March CPS, 0.70 for the OCG-II mailback, and 0.96 for the telephone reinterviews.

The standard errors of these estimates are reportedly about 0.03. Unfortunately, th evidence already cited suggests that the population values in fact differ substantially from these sample values, I doubt that even the apparent discrepancy between the CPS and OCG-II mailback reliabilities would hold for the full OCG-II.

The SRC education measure is somewhat different from the Census Bureau measure, in that it used broader categories and placed more emphasis on degrees. The correlation between reports of educational attainment in 1968 and 1975 for PSID males aged 25-64 in 1968 who were not in school during the interval was 0.92. With random errors the error variance would be 0.86^2 years. These findings suggest that SRC education measures are slightly more accurate than Census measures. For samples with restricted educational variance (e.g. Talent) reliability could be much lower.

4. Father's Education

There are at least four different approaches to estimating the reliability of men's reports of their father's education.

1. Ask men the same question on two different occasions and correlate the results. This is analogous to a test-retest reliability.

2. Ask two brothers with the same father how much education he had and correlate the results. This is analogous to an inter-rater reliability. It should yield a lower value than the test-retest method, since errors are less likely to be correlated.

3. Ask men their father's education, then ask the fathers, and

Correlate the results. If fathers' reports were accurate, this would yield a validity coefficient. Since they are not accurate, the correlation should equal the geometric mean of the two reliabilities.

4. Compare correlations between father's education and father's occupation to correlations between self-reports of education and occupation by the same men. This method is a useful supplement to the previous one.

Reinterviews

Bielby et al. (1976) obtained a test-retest reliability of 0.939 for father's education in the OCG-II reinterview project. This estimate is based on just over 500 non-black males aged 20-64. After examining the pattern of correlations between men's reports of their father's education and their reports of their other characteristics, Bielby et al. concluded that the regular OCG-II mailback questionnaire reports probably had a reliability of about 0.93, while the telephone reinterview reports had a reliability of 0.95. The implied error variance for the mailback questionnaires is 1.12^2 years, which is no larger than for self-reports. Bielby et al. tried to test the hypothesis that the apparent reliability was inflated by respondents' making the same errors in both the mailback questionnaire and the telephone reinterview. But what they appear to have tested was the correlation between errors in reporting one's own education. Their data suggest that errors in reporting one's own education are independent, but it does not follow that this is also true for errors in reporting father's education.

Brothers

Olneck (1976) reports a correlation of 0.777 between Kalamazoo brothers' reports of their father's education (N = 346 pairs). His sample of fathers was somewhat more homogeneous than the OCG sample, however, so his results are not as different from Bielby et al.'s as they first seem. Still, the implied error variance in Olneck's sample was $(3.33)^2(1 - 0.777) = 1.57^2$. Comparing this to Bielby et al.'s estimate yields an F-ratio of $(1.57/1.12)^2 = 1.97$, which is highly significant.

As a check on the validity of this Kalamazoo estimate, I looked at 616 pairs of same-sex twins identified by Schoenfeldt (1968) in the Project Talent sample. The twins were enrolled in grades 9 to 12 in 1960. Both members of 468 pairs provided data on their father's education. This sample had better educated fathers than Olneck's Kalamazoo sample or the OCG sample. (The mean of father's education was 11.7. with a standard deviation of 3.756.) The correlation between twins' reports was 0.820, implying that the error variance was $(3.756)^2(1 - 0.820) = 1.594^2$. This agrees very closely with Olneck's estimate.

These data suggest that reporting errors are about as serious for today's high school students as for older respondents with less education. They also suggest that Bielby et al.'s test-retest reliability is inflated by the respondent's tendency to make the same error in both the questionnaire and telephone reports. **If the error variance is 1.58^2 and the population variance is 4.00, as it is in OCG, the population reliability will be 0.84.**

Father-Son Matching

CPS surveyed men aged 45-59 in 1966 as part of the NLS. Four months later it surveyed men 14-24. The sampling frame was such that 1013 of the 14-24 year olds were sons of the 45-59 year olds. Only 14-24 year olds living at home had fathers in the 45-59 year old sample. Borus and Nestel (1973) found that sons' reports of their father's education correlated 0.954 with the fathers' self-reports (N = 943). If one assumes that fathers' reports are completely accurate, the implied reliability of the men's reports is $0.954^2 = 0.910$, and the error variance is 1.19^2 years. This is comparable to Bielby et al.'s estimate from

OCG. The notion that fathers' reports are completely accurate seems implausible, however. If one treats fathers' and sons' reports as equally accurate, the implied reliabilities are both 0.954, and the error variance is 0.852^2 years. This is far less than the error variance implied by the sibling data, and somewhat less than the error variance implied by Bielby et al.'s results. It is also far less than the error variance for mature men in other CPS data. **It is not clear why the** NLS interviewers got **such** consistent answers from these fathers and sons. **Perhaps** the same interviewer talked to the sons as had talked to the father four months earlier. Interviewers may have rejected responses from sons that did not conform to their recollection of what the father had said. This would inflate the correlation.

Correlation Between Reports of Father's Education and Father's Occupation

Appropriate reliability corrections should make the correlation between sons' reports of father's education and father's occupation equal to the correlation between fathers' self reports of education and occupation. We **could not get** these data for the NLS father-son sample. In their absence, we can simulate the experiment by taking advantage of two facts. First, the correlation between education and occupation does not appear to increase after the age of 30.[1] This means, for example, that the correlation between education and occupation among men 55-64 in 1962 should be a good proxy for the correlation between education and occupation among men 40-49 in 1947. Second, the correlation between education and occupation does not change when one weights by the number of sons men have had.[2] Since a generation is about 30 years,

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[1] This conclusion is based on cohort comparisons between OCG and OCG-II and on 1970 Census data on occupational status in 1965 and 1970.

[2] This conclusion is based on OCG data for men 45-54 in 1962.

this means that the correlation between education and occupation for men aged 40 to 49 in 1947 should be the same as the correlation between father's education and father's occupation for sons born1927 -1936. These sons would have been 10 to 19 in 1947 and 25-34 in 1962. Thus if 25-34 year old sons' retrospective reports of their father's education and occupation were as reliable as their fathers' reports, the correlation between reports of father's education and father's occupation for OCG men aged 25-34 in 1962 would equal the correlation between self-reports of education and occupation for OCG men aged 55-64 in 1962. In fact, the correlation between sons' reports on their father was 0.489, whereas the correlation between the fathers' self-reports was 0.567. These values have standard errors of about 0.02 so the difference is statistically significant. It seems fair to conclude that sons' reports of their father's education and occupation are not as reliable as self-reports.

We can formalize this argument by designating self-reports of father's education as U, self-reports of father's occupation as X, sons' reports of father's education as U', sons' reports of father's occupation as X', and the reliabilities of these reports as r_{UU}, r_{XX}, $r_{U'U'}$ and $r_{X'X'}$ respectively. Then if we designate the true correlation between X and U as r^*_{XU}, and if we assume that errors in reporting X and U are independent, we can show that

$$(16)\quad r_{XU} = r^*_{XU}\,(r_{XX}r_{UU})^{1/2}$$

and

$$(17)\quad r_{X'U'} = r^*_{XU}(r_{X'X'}r_{U'U'})^{1/2}.$$

Dividing equation 17 by equation 16 and rearranging gives us:

$$(18)\quad \left(\frac{r_{X'X'}r_{U'U'}}{r_{XX}r_{UU}}\right)^{1/2} = \frac{r_{X'U'}}{r_{XU}}$$

If we assume the values suggested above, this yields:

$$(19)\quad \left(\frac{r_{X'X'}r_{U'U'}}{r_{XX}r_{UU}}\right)^{1/2} = \left(\frac{0.489}{0.567}\right) = 0.862$$

If we also assume that the relative reliability of education and occupational status is the same for reports on fathers as for self-reports,

$$(20)\quad \frac{r_{XX}}{r_{UU}} = \frac{r_{X'X'}}{r_{U'U'}}$$

Solving for $r_{X'X'}$ and substituting in equation 18 then yields:

$$(21)\quad r_{U'U'} = \frac{r_{UU}\, r_{X'U'}}{r_{XU}}$$

If we assume that the CPS reliability for self-reported education is about 0.90, we obtain:

$$(22)\quad r_{U'U'} \cong \frac{(0.90)(0.489)}{0.567} \cong 0.78$$

The implied error variance in father's education for OCG 25-34 year old men is thus about $(1-0.78)(3.818)^2 = 1.79^2$ years. If the CPS reliability is 0.85, the reliability of reports on father s is 0.73 and the error variance is 1.97^2.

In summary, the OCG-II reinterview study implies a reliability of 0.93 for reports of father's education, while the NLS match of fathers' and son's reports implies a reliability between 0.91 and 0.954. The two sibling studies imply that the reliability for representative samples of men 25-64 must be about 0.84. Correlations between reports of father's education and father's occupation imply a reliability between 0.73 and 0.78. Having already estimated the reliability of self-reported education at 0.85 to 0.90, I am reluctant to accept estimates in excess of 0.90 for reports of father's education. I am therefore inclined to discount the OCG-II reinterview data and the NLS data. The OCG-II reinterview data tell us that respondents' reports of their father's education are temporally stable. The low correlation between brothers' reports suggests that part of this stability is due to correlated errors. The NLS estimate is hard to reconcile with either the OCG-II data or the sibling data. Perhaps the NLS data are contaminated. Alternatively, they may be of unusually high quality. If the NLS results were generally applicable, there would be no obvious explanation either for the relatively low correlation between brothers' reports or for the depressed correlation between reports of father's education and father's occupation. In light of this, I will proceed on the assumption that sibling data provide the best basis for estimating the reliability of reports on fathers' characteristics. The implied reliability of reports of father's education is then about 0.84.

4. Father's Occupation

I used the same four methods to estimate the reliability of father's occupation as to estimate the reliability of father's education.

Reinterviews

Bielby et al. obtained a correlation of 0.869 between the mailback questionnaire and telephone reports of father's occupation for 500-odd non-black males aged 20-64 in the OCG-II reinterview project. After analyzing the overall pattern of correlations, they concluded that the reliability of the mailback questionnaire item was 0.85, while the reliability of the telephone item was 0.89. They estimated the error variance of the mailback questionnaire reports as 9.37^2 points.

Brothers

Olneck (1976) obtained a correlation of 0.765 between 409 pairs of brothers' reports of their father's occupation in Kalamazoo. This correlation is not precisely equivalent to an inter-rater reliability, since Olneck asked each brother to report his father's occupation when the brother in question was 16. Since brothers' ages differed by an average of 4.5 years, some fathers could really have changed their occupations between the time the first brother reached 16 and the time the second brother reached 16. The 1970 Census, for example, asked men about their jobs in both 1970 and 1965. Those who had the same job presumably said so, eliminating random errors for non-changers. Nonetheless, the correlation between Duncan scores in 1965 and 1970 for men 40 to 49, was only 0.90. **In PSID, the correlation was 0.826 over five years. Correcting for unreliability, the implied "true" correlation was 0.868. Assuming the correlation over a 4.5 year inter-**val was 0.91, the population variance due to real changes over a 4.5 year interval would be about $(1 - 0.91)(25)^2 = 7.5^2$ points. The total variance not common to brothers in Olneck's sample was $(1 - 0.765)(22.52)^2 = 10.92^2$ points. If 40-49 year olds changed occupations as much in Kalamazoo as elsewhere, the error variance for Kalamazoo brothers' reports would be $10.92^2 - 7.5^2 = 7.93^2$ points. But occupational shifts were probably not as common in the Kalamazoo sample as in the nation as a whole, since the Kalamazoo sample includes few geographically mobile fathers, and geographic mobility tends to correlate with occupational mobility. I infer that the error variance for the Kalamazoo sample exceeds 7.60^2 but is less than 10.68^2. The Kalamazoo error variance is thus potentially compatible with Bielby et al.'s error variance.

The Talent twins' reports of their father's occupation are not entirely comparable to Olneck's or Bielby et al.'s measures, since the twins did not actually describe their father's job. Instead, they assigned their father directly to one of 16 broad occupational groups. We then assigned the father our best estimate of the mean Duncan score of his group. The correlation between twins' reports (N = 481 pairs) is 0.856. The error variance is $(1 - 0.856)(23.252)^2 = 8.824$ points which is slightly smaller than Bielby et al.'s estimate. This may **mean that** respondents have a clearer idea of their father's current occupation when they are in high school than when asked to supply retrospective information about his occupation 30 years later.

Father-Son Correlations

After eliminating all men who might have changed occupations between interviews, Borus and Nestel obtained a correlation of 0.89 between fathers' and sons' reports of the father's occupation. If one assumes that the father's report is completely accurate, which seems unlikely, the implied reliability for sons' reports is $0.89^2 = 0.79$. If one assumes that both fathers' and sons' reports are equally fallible, the implied reliability of both fathers' and sons' reports is 0.89. In any event, the implied reliability of the fathers' self-reports is higher than in most other surveys. If one assumes that fathers' and sons' reports are equally accurate, the implied error variance is $(1 - 0.89)(24.12)^2 = 8.00^2$. If fathers' reports are more accurate than sons', the error variance in sons' reports could be as high as 11.00^2.

Correlations between Father's Education and Father's Occupation

Solving equation 19 for $r_{X'X'}$ and assuming $r_{XX} = 0.85$, $r_{UU} = 0.90$, and $r_{U'U'} = 0.78$, the implied reliability of father's occupation for men 25-34 is 0.73. The implied error variance for our OCG sample is $(1 - 0.73)(21.417)^2 = 11.1^2$ points.

The estimated standard error of father's occupation thus ranges from 8 to 11 points, depending on the sample and estimating procedure. Since the standard deviation of father's occupation is 21 points our OCG sample, the implied reliability falls between $1 - (8/21)^2 = 0.85$ and $1 - (11/21)^2 = 0.73$. This is a considerable margin of error.

Since no one estimation procedure seems clearly superior to the others, one should probably assume an intermediate standard error, such as 9.5.

In a sample with an observed variance of 21 points, this implies a reliability of 0.80.

This estimate applies to reports that are coded into detailed Census categories and then assigned a Duncan score. When reports are coded into broad categories, the reliability may be slightly higher, though we cannot be sure of this.

6. Siblings

Olneck obtained a correlation of 0.942 between Kalamazoo brothers' reports of their number of siblings. The implied error variance was $(1 - 0.942)(2.53)^2 = 0.61^2$ siblings. If this level of error prevailed in OCG-I as well, the implied reliability for men 25-64 with income would be 1 - (0.61/3.20) = 0.96.

Chapter 14

The Effects of Selected Sample Restrictions

By Christopher Jencks

The analyses in this report cover economically active males between the ages of 25 and 64 who were not in school, not in the military, and not in institutions at the time they were surveyed. The multivariate analyses are further restricted to men who provided complete data on the items that interested us. This chapter explores the effects of these restrictions. It begins by analyzing the factors that determine whether an individual is economically active and the effects of excluding economically inactive men from our analyses. It then turns to the effects of excluding students, soldiers, inmates, men under 25, and men over 64. It does not analyze the effects of excluding women. I hope to repair this gap at a later date. The chapter concludes by looking briefly at the effect of omitting non-respondents.

1. Labor Force Participation

An exhaustive investigation of the determinants of labor force participation would require a book in itself. My aim here is merely to assess the effects of omitting non-participants from our analyses. To answer this question I will investigate what effect, if any, race, Southern birth, education, and experience had on a man's chances of working for pay in 1969, using 1970 Census data. It would be useful to extend this analysis to include other background characteristics, test scores and personality traits, but I did not attempt this.

Table 14.1 shows regression equations from two 1970 Census samples. The first sample covers all men aged 14 to 99 in 1970. (Men coded as aged 100 or more have defective records on **the** 1/1000 **tapes.)** 78.65 percent of all men 14-99 reported positive earnings in 1969, 0.22 percent reported negative earnings (from self-employment), and 21.13 percent reported no earnings whatever. The first four equations in Table 14.1 estimate the probability that individuals with various characterics reported positive 1969 earnings. Like all **regressions** with dichotomous dependent variables, these equations **can in theory** **predict proportions** greater than 1.00 or less than zero. In practice, this is a minor problem. The constant in equation 1 implies that among men who were neither white nor Northern the proportion with earnings in 1969 was 0.7111. For those who were Northern but nonwhite, the estimated proportion was 0.7111 + 0.00616 = 0.71727. For those who were both white and Northern, the estimated proportion was 0.7111 + 0.00616 + 0.07937 = 0.79664. I have not bothered to report standard errors, since they are all less than 0.01.[1]

Equation 2 shows that the proportion of men with positive earnings rises by 0.037 as education rises one year. This is partly because respondents with little education are **more** often still

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[1] Since the dependent variable is dichotomous, the residuals are not normally distributed. The standard errors can therefore be misleading.

Table 14.1

Regression of "Worked for Pay" in 1969 on Selected Characteristics of 1970 Census Respondents:

All Males 14-99 except those with allocated values on Age

INDEPENDENT VARIABLE	Men 14-99 (N=69,912)				Men 25-64 (N=42,587)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
White	.07973	.02814	.04060	.04522	.05963	.04107	.04549	.03534
Non-South Upbringing	[.00616]	[-.03062]	[-.00409]	[.00131]	.01627	[.00096]	[.00384]	[-.00111]
Education		.03668	.02243	.06673		.01186	.00790	.01302
Experience			.02577	.04906			.00750	[.00184]
$Experience^2$			-.00051	-.00053			-.00020	.00029
$Experience^3$				[-.00000]				-.00001
Education * Experience				-.00125				-.00012
Education Past High School				-.02553				.00835
BA				-.05520				[.00043]
No Disability				.19571				.22752
Not in School				.47865				.18392
Not in School*Age				-.01256				-.00370
Institutionalized				-.36690				-.53892
Constant	.71111	.38744	.37181	-.42561	.86701	.76024	.76365	.42301
$\bar{R}^2$	.00389	.10612	.29257	.41023	.00709	.03608	.06081	.26563
S.D. of Residuals	.40901	.38745	.34469	.31472	.25146	.21776	.24456	.21620

[Coefficients in brackets are less than twice their standard errors.]

in school. Equation 3 is more instructive. It shows that with Experience and Experience^2 controlled, each extra year of education increases the proportion of men with earnings by 0.02243. The coefficients of Experience and Experience^2 show the familiar curvilinear pattern. The probability of having earnings rises during the first years of experience, peaks after 0.02577/(2) (0.00051) = 25 years, and then diminishes at an accelerating rate. This curve is almost identical to the curve predicting Ln Earnings among labor force participants.

Equation 4 shows that both additional education and additional experience substantially increase the chance of having earnings if one has not completed high school, but that neither has much effect on those who have finished high school. The reason is simple. Among 25 year old high school graduates who were not in school, institutionalized, or disabled, an estimated 99.45 percent had earnings in 1969. Thus there was little room for further improvement as they acquired more experience.

Equation 4 also shows that men who were in school or in institutions, or who were disabled, were appreciably less likely to have earnings. The negative coefficient of the interaction between school attendence and age indicates that the impact of school attendence diminishes with age. For a 14 year old, school attendence decreases the probability of having earnings by 0.47865 - (14)(0.01256) = 0.30281. For a 34 year old, the decrease is only 0.47865 - (34)(0.01256) = 0.05161. The equation implies that school attendence actually increases the chances of having earnings among men over 38. This implication is almost certainly misleading.

It indicates that the equation makes inadequate allowance for nonlinear interactions.

The right side of Table 14.1 shows regressions restricted to 25-64 year olds. This restricted sample eliminates certain nonlinearities and interactions that were not adequately specified in the equations for all men 14 to 99. In this sample 93.16 percent of all men reported positive earnings in 1969, 0.30 percent reported negative earnings from self-employment, and 6.54 percent reported no earnings. The effects of education are only a third as large for 25-64 year olds as for 14 to 99 year olds. The reason for the change is that mean labor force participation is much higher for 25-64 years olds. As a result, additional education cannot increase participation as much. An extra year of school raises the proportion of men with positive earnings by only 0.0079.

Equation 4 shows that elementary and secondary education again have more impact than higher education. School enrollment again lowers labor force participation, but not much. Among 25 year olds, school attendence lowers the proportion with earnings by 0.09142. Among 35 year olds the reduction is only 0.05442. The implied effect of school enrollment does not become positive until the age of 50. This may still be unrealistic, but given the small number of students over 50, the error is probably insignificant.

The reader should bear in mind that characteristics like school enrollment, institutionalization, and disability are ascertained as of March, 1970, whereas earnings are ascertained for 1969. The effect of measured characteristics on the chances of having earnings is therefore understated to the degree that the characteristics in question changed between 1969 and 1970.

The effect of a given trait on economic success is the weighted sum of its effect on a man's chances of working and its effect on his success if he works. Looking only at a trait's effect on success of those who work may therefore distort its overall effect. We can illustrate this by considering the effects of race on occupational status. If w is the proportion of whites who work, n is the proportion of non-whites who work, $\bar{Y}_w$ is the mean status of whites who work, $\bar{Y}_n$ is the mean status of non-whites who work, and Y_u is the status of individuals who do not work, the true difference (D_t) between whites and non-whites is:

$$(1) \quad D_t = \left[w\bar{Y}_w + (1-w)Y_u\right] - \left[n\bar{Y}_n + (1-n)Y_u\right] = w\bar{Y}_w - n\bar{Y}_n - (w-n)Y_u$$

If we look only at those who work the observed difference (D_o) will be:

$$(2) \quad D_o = \bar{Y}_w - \bar{Y}_n$$

The bias in this estimate is therefore:

$$(3) \quad D_o - D_t = (1-w)\bar{Y}_w - (1-n)\bar{Y}_n + (w-n)Y_u$$

Unless we know the status of individuals who do not work, we cannot estimate either the true difference or the bias. [illegible] we are willing to assign non-workers a status, we can calculate both quantities. If, for example, we treat men without jobs as having Duncan scores at the bottom of the scale (i.e. 0), we obtain:

(4) $D_o - D_t = (1-w)\bar{Y}_w - (1-n)\bar{Y}_n$

This bias need not be negative, even if w is greater than n. If non-workers have the same status regardless of race, and if racial differences in the proportion of non-workers are smaller than racial differences in status among workers, looking only at workers will overstate the overall difference between races rather than understating it. In the present instance, $D_o - D_t$ is roughly (0.06)(41) - (0.13)(25) = -0.8 points. But if only 80 percent of whites and 75 percent of non-whites had worked, the bias would have been (0.20)(41) - (0.25)(25) = 1.95 points The status gap between blacks and whites who worked would thus have been larger than that between blacks and whites generally.

One can easily make similar computations for other determinants of occupational status. Appendix A suggests, for example, that men with 8 years of school have Duncan scores of about 28 while men with 12 years of school have scores of about 42. Among white Northern men aged 25-64, equation 6 shows that labor force participation averaged 89.7 percent for men with 8 years of schooling and 94.4 percent for men with 12 years of schooling. Looking only at labor force participants thus overestimates the benefits of high school by (1-0.897)(28) - (1-0.974)(42) = 0.5 points, which is hardly a consequential bias. The bias is in the opposite direction for higher education, but is again tiny.

One can also make such calculations for earnings. Including men who do not work and assigning them zero earnings does not appreciably alter our results so long as the dependent variable is actual earnings (see Chapter 16). If the dependent variable is Ln Earnings, the inclusion of men without earnings leads to nonsense results.

Differential labor force participation has another consequence that deserves comment. When we estimate the benefits of education, cognitive skills, or family background, we typically make estimates for a single year. But we are often interested in lifetime benefits. A trait can affect lifetime earnings either by affecting average earnings in the years men work or by affecting the total number of years men work. Precise estimates of a trait's impact on the number of years men work depend on the probability that men with a given trait will survive to a given age and the probability that survivors will work at that age. The product of these two values is the probability that men with a specified trait will have earnings at a given age. If we assume that these cross-sectional probabilities also apply to a single cohort followed through time, their sum gives us the expected working life of men who have a given trait.

Kitagawa and Hauser (1973) provide differential mortality data for men with various socioeconomic characteristics, but not in the detailed form needed for constructing the matrices described above. Fortunately they also estimate the mean difference in life expectancy between men with certain characteristics. The bottom lines of Table 14.2 show their estimates for men with varying amounts of education. 25 year old men with

Table 14.2

PERCENTAGE OF MEN WHO REPORTED POSITIVE EARNINGS DURING 1969 BY AGE AND EDUCATION: 1970 CENSUS 1/1000 SAMPLE OF MEN 14-99 REPORTING RELEVANT VARIABLES (N=62,724)

AGE IN 1970	YEARS OF EDUCATION 0-7	8	9-11	12	13-15	16	17	ALL
14-19	15.8	21.9	52.9	85.2	90.8	-	-	48.9
20-24	78.6	81.4	91.0	94.1	90.6	94.3	88.2	91.5
25-29	80.4	93.8	96.0	97.9	95.7	97.3	95.6	96.1
30-34	87.4	95.7	96.4	98.1	97.4	99.1	97.1	96.9
35-39	84.7	95.5	96.9	97.6	98.6	98.6	99.3	96.6
40-44	88.1	94.3	96.6	97.4	97.7	99.3	99.5	96.3
45-49	87.7	94.1	95.4	96.2	96.8	99.0	99.5	95.3
50-54	83.9	92.7	93.8	95.1	95.4	95.2	98.1	93.0
55-59	79.9	90.3	92.4	92.6	94.8	94.3	96.8	90.1
60-61	77.8	84.8	85.2	88.1	93.1	[93.3]	[91.8]	85.4
62-63	66.9	78.4	79.7	81.3	87.6	[87.1]	[91.0]	78.2
64-65	56.6	70.3	68.4	74.2	78.6	[79.7]	[77.4]	67.9
66-67	47.6	50.2	49.1	58.7	[67.7]	[60.0]	[72.7]	52.7
68-69	30.8	41.4	41.6	47.4	[39.7]	[34.4]	[56.8]	39.1
70-71	29.2	31.9	28.9	[39.8]	[44.4]	[39.3]	[46.7]	33.0
72-73	19.9	28.2	[33.3]	[32.9]	[40.3]	[39.4]	[43.8]	28.1
74-79	13.2	15.4	21.7	19.7	[17.5]	[42.1]	[39.5]	17.3
80-84	4.9	10.3	[7.0]	[22.2]	[14.3]	[5.0]	[33.3]	9.1
85-89	0.9	[8.8]	[0]	[0]	-	-	-	4.0
90-94	[0]	-	-	-	-	-	-	[2.8]
95-99	-	-	-	-	-	-	-	-
Mean Years Worked by Survivors from 25 to 64	33.1	36.8	37.4	37.8	38.2	38.5	38.7	37.
Mean Years Worked by Survivors from 65 to 73	3.1	3.7	3.7	4.3	4.6	4.3	5.2	3.7
Life Expectancy at 65 1/	12.8	13.0	13.5	12.9		13.1		
Life Expectancy at 25 1/	43.7	44.8	45.6	46.0		47.1		

Percentages based on less than 100 cases are in brackets.
Percentages based on less than 20 cases are omitted.
1/ Data from Kitagawa and Hauser (1973).

16 years of school can expect to live 47.1 more years, while 25 year old men with 8 years of school can only expect to live 44.8 more years. The previous line of Table 14.2 shows that if men with different amounts of schooling reach the age of 65, their life expectancies are virtually identical. The difference between them, then, is almost entirely due to differential mortality between 25 and 64. Since relatively few men retire much before 65, differential morality between 25 and 64 means differential labor force participation as well. Differential mortality will thus make the working lives of men with 8 years of school about 2.2 years shorter than the working lives of men with 16 years of school.

Table 14.2 also shows that survivors are less likely to work if they are poorly educated than if they are well educated. This is not primarily because the highly educated retire later. If we define the median age of retirement as 1.3 years prior to the age at which 50 percent of Census respondents report no earnings for the previous calendar year, interpolation suggest that it falls at 65.7 for men with 8 years of schooling and 66.5 for men with 16 years of schooling. But while the median retirement age is very similar at all educational levels, Table 14.1 made clear that labor force participation prior to retirement varies significantly by education. Table 14.2 confirms this. The fourth row from the bottom shows that men with 8 years of school who survive from 25 to 64 work in 36.8 of these 40 years. Men with 16 years of school work in 38.5 of those years. Those with less than 8 years of schooling work only 33.1 years. The effects of education on labor force participation are thus strongly non-linear.[2] Men who failed to complete elementary school are quite likely to spend a substantial number of years outside the labor force. Men who manage to complete elementary school usually have the basic competencies needed to find and keep a job of some sort, and most do so. As a result, additional schooling cannot greatly increase their chances of working. It can only increase their probable earnings.

An additional year of schooling above 8th grade is typically associated with an 0.3 year increase in life expectancy and an increase of 0.2 years in the working life of survivors between 25 and 64. The increase in working life of survivors is 0.1 years between 65 and 73. These effects are not fully additive, but each extra year of education is almost certainly associated with at least an 0.5 year increase in the number of years in which a 25 year old can expect to work.[3]

The next question is how much an extra year of school reduces a man's working life prior to 25. Among men 14 to 24, 49.4 percent of those who were enrolled in school in March 1970, had 1969 earnings, compared to 84.3 percent of those not in school. This suggests that an extra year of school attendence typically leads to 0.349 fewer years with earnings prior to 25. Weighing this against the 0.5 years gained after the age of 25, I conclude that a year of school attendence typically increases the total number of years in which a man has earnings between the ages of 14 and 99 by between 0.1 and 0.2. This is close enough to zero so we can ignore it for most purposes.

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[2] This non-linearity is apparent in equations 4 and 8 of Table 14.1. but it is not fully captured by the specification used there.

[3] This estimate appears to be appreciably lower than Mincer's (1974). We have not explored the reasons for the discrepancy.

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effects of differential mortality and differential labor force participation instead of multiplying them. They also treat all years of work as identical, when in fact some years represent substantially more weeks and hours than others. Census data suggest, for example, that male students 14 to 24 who worked in 1969 averaged 23 weeks in the labor force while non-students who worked averaged 40 weeks. Furthermore, students who worked during March, 1970, averaged 21 hours per week while non-students averaged 42 hours.[4] After school completion, however, extra education usually leads men to work more weeks in a given year. Despite these difficulties, it seems reasonable to conclude that education has little impact on the total number of years during which men work, and that its overall impact on the number of weeks they work is relatively modest.

One could make analogous calculations to see whether these conclusions with respect to education apply to race, **test scores, personality traits, and so forth, but I have not made them.**

2. Students, Soldiers, and Inmates

Wherever possible we eliminated men who were enrolled in school full-time, who were in the military, or who were in institutions. Our rationale was that the/current occupational status and earnings of these men are were poor indicators of **both** current well-being / and likely lifetime success. **All three groups receive substantial unrecorded income. Students often get transfers from their parents,** schools, or government. Students also look forward to substantially higher earnings once they finish school. Most soldiers were in a similar position during the period covered by our surveys, since they were drafted for only two years and paid far less than their market wage. They too could expect much higher earnings after returning to civilian life. Inmates seldom have any earnings while they are institutionalized. Their earnings for the previous year, if any, are likely to cover only part of the year. They, too, will usually earn more if and when they are released.

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[4] One should not multiply weeks worked by hours of work to estimate annual hours of student employment, because students are more likely to be working part time in March than during the summer.

Excluding students, soldiers, and inmates can make a substantial difference in samples that include men under 25. It is less consequential when we look only at 25-64 year olds. Table 14.3 gives the percentage in each group that worked during 1969, the number of weeks they typically worked, and the number of hours they worked per week, broken down by age. The difference between students and non-students is clearly substantial for men 14-24. It is much smaller for men 25-64. In any event, only 2.4 percent of all men 24-64 said they were enrolled in school at the time of the 1970 Census. Soldiers do not differ much from their civilian counterparts on the indices in Table 14.3, but soldiers reported mean earnings of only $91 per week for 1969, compared to $153 for civilians who were not in school or in institutions. Less than two percent of all men 25-64 reported being in the military. Inmates are dramatically different from the rest of the population, but they constitute only 1.3 percent of all men 25-64.[5]

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[5] Ideally, we would like to exclude men who were in school, in the military, or in institutions at any time during the year covered by our earnings data. Our surveys did not collect such retrospective information, however. Our longitudinal surveys provide data on the respondent's status at some point during the previous year, but if a respondent was in the military, or in school, or in an institution during part of the previous year but not all of it, our longitudinal surveys might not tell us this.

Table 14.3

PERCENTAGE OF MALES WHO WORKED FOR PAY, MEAN WEEKS WORKED, AND MEAN HOURS WORKED, BY AGE: 1970 CENSUS 1/1000 SAMPLE OF MEN REPORTING RELEVANT VARIABLES

AGE & STATUS IN MARCH, 1970	N	PERCENT WITH 1969 EARNINGS	MEAN WEEKS WORKED IN 1969 BY THOSE WHO WORKED	MEAN HOURS WORKED IN LAST WEEK OF MARCH, 1970, BY THOSE WHO WORKED
14-24	17,686	64.9	32.7	33.7
Student	10,297	49.9	23.0	20.9
Military	1,162	94.9	43.6	NA
Institutionalized	194	27.8	22.9	NA
Other	6,033	86.1	40.1	41.5
25-69	39,698	93.7	48.0	44.4
Student	964	91.5	43.6	39.0
Military	780	94.9	49.6	NA
Institutionalized	491	20.6	31.3	NA
Other	37,463	94.7	48.2	44.5
65- 99	7,601	31.2	37.2	35.4
Student	41	24.4	44.0	35.1
Military	5	20.0	7.0	NA
Institutionalized	287	2.1	35.5	NA
Other	7,268	32.4	37.2	35.4
All	64,985	78.6	44.1	42.2

Note: The Census measure of school enrollment does not distinguish part-time from full-time students. Thus the difference between "students" and "non-students" is less than in some of our other samples which treat part-time students like non-students. On the other hand, these samples have substantially fewer 25-64 year old "students" than the Census.

Table 14.4 displays two sets of regressions, one covering our usual sample of civilian, non-student, non-institutional males aged 25-64, the other covering a sample of students, soldiers, and inmates aged 25-64. Both samples are restricted to men with positive earnings in 1969 and complete data on all relevant variables. Both come from the 1970 Census. The first equation for each sample is the regression of LnEarnings on race, region of birth, three education measures, and two experience measures. The regression coefficients for the two samples differ, but the differences never exceed twice their sampling error. The constants for the two samples differ dramatically. This reflects the fact that the equation for students, soldiers, and inmates includes dummies for being a student or a soldier, so the constant for this sample is based on the earnings of inmates. But the earnings of students and soldiers are also substantially lower than for our usual sample. The unexplained variance of LnEarnings is also larger

Table 14.4

Regressions of 1969 Earnings on Selected Characteristics of 25-64 Years Old Males with Positive Earnings and Complete Data, by Institutional Status in 1970: Census 1/1000 Sample

INDEPENDENT VARIABLE		CIVILIAN NON-STUDENT NON-INSTITUTIONAL (N=25,697)		MILITARY STUDENT OR INSTITUTIONALIZED (N=1291)	
		Ln Earnings	Ln Weekly Earnings	Ln Earnings	Ln Weekly Earnings
White	B	.284	.249	.381	.299
	(S.E.)	(.015)	(.014)	(.075)	(.058)
Non-Southern Birth	B	.116	.122	[-.011]	[.010]
	(S.E.)	(.009)	(.008)	(.051)	(.039)
Education	B	.073	.060	.108	.087
	(S.E.)	(.002)	(.002)	(.019)	(.015)
Education Past High School	B	[-.007]	.010	-.075	-.041
	(S.E.)	(.006)	(.005)	(.030)	(.023)
BA	B	.124	.111	.143	[.094]
	(S.E.)	(.026)	(.023)	(.108)	.083)
Experience	B	.042	.034	.070	.055
	(S.E)	(.001)	(.001)	(.008)	(.006)
Experience^2	B	-.00075	-.00057	-.0012	-.00098
	(S.E.)	(.00003)	(.00003)	(.0002)	(.00016)
Student	B	-	-	5.71	.268
	(S.E.)			(.096)	(.074)
Military	B	-	-	.543	.093
	(S.E.)			(.095)	(.073)
Constant		7.311	3.690	5.859	2.877
$\bar{R}^2$		.198	.200	.170	.175
S.D. of Residuals		.641	.575	.780	.600

for students, soldiers, and inmates. The exclusion of students, soldiers, and inmates therefore reduces both the variance of LnEarnings and the variance of the residuals in our samples. It does not appear to alter our regression coefficients to any appreciable degree.

The second equation for each sample substitutes weekly earnings for annual earnings as a dependent variable. This makes the unexplained variance for students, soldiers, and inmates very similar to that for our usual sample.

3. Age Restrictions

We restricted all our analyses to men between 25 and 64. The rationale for this was that men 25-64 are more strongly committed to the labor force than older or younger men. Table 14.2 indicated that they are more likely to work for pay. Table 14.3 indicated that they are also more likely to work full-time, year-round. This is partly because employers are more willing to hire them. But it is also partly because they are under more social and economic pressure to pursue a career during these years. Strong labor force attachment should minimize the variance of motivational factors, such as preference for leisure rather than cash, and should raise the correlation of "human capital" measures with earnings. It should also raise the correlation between current earnings and lifetime earnings, at least if age explains a substantial fraction of the variance in motivation. Unfortunately, we have not gotten very far into our exploration of lifetime income inequality, so this argument is entirely conjectural.

We have, however, explored the effect of omitting men under 25 and over 64 from our regression analyses. Table 14.5 shows the regressions of Occupation, LnEarnings, and LnWeekly Earnings on race, region of birth, educational attainment, and post-educational experience for men 14-24, 25-64, 65-99, and 14-99. The first column gives the mean and standard deviation of each measure of economic success for each group. The means follow the familiar curvilinear pattern, starting low, peaking in middle age, and falling in old age. Comparing the means for Ln Earnings to those for LnWeeklyEarnings, and recalling that LnWeeks Worked = LnEarnings - Ln Weekly Earnings, we can see that LnWeeks Worked must also be substantially higher for men 25-64 than for older or younger men. This is consistent with Table 14.3, which showed that men 25-64 worked an average of 48 weeks per year if they worked at all, while men 14-24 worked 33 weeks, and men 65-99 worked 37 weeks.

The first column of Table 14.5 also shows that the standard deviation of occupational status is restricted for 14-24 year olds but not for 65-99 year olds. The standard deviation of LnEarnings, in contrast, is higher for 14-24 year olds and 65-99 year olds than for 25-64 year olds. This is partly because fewer 14-24 year olds and 65-99 year olds work all year. When we look at LnWeekly Earnings, the difference between the standard deviations for 14-24 year olds and 25-64 year olds is quite small. The difference between 25-64 year olds and 65-99 year olds is still substantial. These remaining discrepancies might disappear if we could control mean hours worked per week in 1969. They do not disappear, however, if we merely control hours worked during the last week of March, 1970.

Turning to the regression results, we look first at occupational status. With education controlled, having a white skin provides a smaller advantage to men 14-24 than to older men. This is

Table 14.5

REGRESSIONS OF SELECTED MEASURES OF SUCCESS ON RACE, REGION OF BIRTH, EDUCATION, AND EXPERIENCE FOR CENSUS RESPONDENTS AGED 14-24, 25-64, and 65-99 WITH POSITIVE 1969 EARNINGS, COMPLETE DATA, NOT IN SCHOOL, NOT MILITARY, AND NOT IN INSTITUTIONS

DEPENDENT VARIABLE & AGE	Mean and (S.D.) of Dependent Variable	Regression Coefficients of Independent Variables (with Standard Errors)								
		White	Non-South Birth	Education	Education past H.S.	B.A.	Experience	Experience^2	Constant	S.D. of Residuals ($\bar{R}^2$)
upation										
14-24 N=3864)	30.400 (20.391)	4.621 (.948)	[.843] (.620)	2.689 (.218)	4.173 (.511)	7.957 (1.988)	2.415 (.476)	-.143 (.060)	-13.793	17.058 (.300)
25-64 N=25697)	40.794 (24.543)	8.441 (.444)	[.084] (.267)	2.950 (.060)	2.587 .164)	4.021 (.763)	.249 (.042)	-.0028 (.0008)	-8.608	18.645 (.423)
65-99 N=1630)	35.918 (24.989)	7.596 (1.975)	[-.419] (1.159)	2.684 (.205)	2.290 (.798)	[-2.542] (3.761)	-4.044 (1.158)	.037 (.010)	112.346	19.805 (.372)
All N=31191)	39.252 (2[illegible].344)	7.842 (.397)	[.176] (.242)	2.896 (.054)	2.784 (.152)	4.480 (.703)	.473 (.024)	-.0057 (.0005)	-11 345	18.599 (.416)
Earnings										
14-24	8.076 (.995)	.294 (.048	[.022] (.032)	.234 (.011)	-.132 (.026)	[.151] (.101)	.387 (.024)	-.01798 (.00304)	4.224	.869 (.237)
25-64	8.981 (.716)	.284 (.015)	.115 (.009)	.073 (.002)	[-.006] (.006)	.124 (.026)	.042 (.001)	.00075 (.00003)	7.311	.642 (.198)
65-99	7.972 (1.286)	[.217] (.119)	[.070] (.070)	.047 (.012)	[-.068] (.048)	[.153] (.227)	-.257 (.002)	.00181 (.00063)	15.757	1.195 (.135)
All	8.816 (.871)	.280 (.016)	.103 (.010)	.074 (.002)	[.002] (.006)	.150 (.028)	.080 (.001)	-.00143 (.00002)	6.856	.736 (.287)
Weekly Earnings										
14-24	4.508 (.706)	.168 (.036)	.080 (.024)	.133 (.008)	-.057 (.020)	.110 (.076)	.173 (.018	-.00681 (.00230)	2.355	.656 (.139)
25-64	5.125 (.643)	.249 (.014)	.122 (.008)	.060 (.002)	[.010] .005	.111 (.023)	.034 (.001)	-.00057 (.00003	3.690	.575 (.200)
65-99	4.532 (1.076)	.260 (.100)	.130 (.059)	.027 (.010)	-.020 (.041)	.095 (.191)	-.197 (.059)	.00138 (.00053)	10.432	1.006 (.125)
All	5.018 (.719)	.235 (.013)	.119 (.008)	.059 (.002)	.015 (.005)	.120 (.024)	.053 (.001)	-.00092 (.00002)	3.473	.622 (.251)

partly because very young whites are seldom in high status occupations. This reduces the potential effects of race on status among the young. In addition the small coefficient for young men reflects secular improvement in the position of non-whites, which has affected the young more than their elders. Southern birth is consistently inconsequential with education controlled. An extra year of elementary and secondary education has slightly more effect (measured by the coefficient of Education) for men 25-64 than for younger or older men, but the difference between samples is not significant. Both a year of higher education and a BA have more impact on men 14-24 than on younger or older men. This is consistent with the pattern shown in Appendix A for cohorts aged 25-34, 35-44, 45-54, and 55-64. The effects of higher education on occupational status are stronger for younger men. This could imply a secular increase in the effects of education, combined with a segmented labor market that allowed older men to retain high status jobs even if they had less education than young applicants. Alternatively, the status benefits of higher education may decline as individuals get older. We have not explored this issue in detail.

The coefficients of Experience and Experience^2 differ dramatically from one age group to another. While the effects of experience are clearly curvilinear, the variation in coefficients for samples of varying age means that the overall curve is not a parabola. It rises rapidly during the first years of experience, but at a sharply decelerating rate. The rate of deceleration in turn declines, implying that one needs at least a cubic to capture the trend. The estimated curve among men over 65 is concave upward instead of downward. As noted in Chapter 12, then, Experience and Experience^2 will not estimate the marginal returns to a given year of experience very accurately in samples with a wide age range. Both the initial returns to experience are understated and the eventual costs are overstated by the quadratic specification.

When we turn from occupational status to LnEarnings, the percentage increase in earnings associated with a white skin is as large for men 14-25 as for older men once education is controlled. Comparing the equations for Ln Weekly Earnings to those for Ln Earnings we can see that an appreciable part of the earnings gap between 14-24 year old whites and non-whites with similar education derives from the fact that whites worked more weeks during 1969 than equally educated non-whites. This is far less true for older whites and non-whites. The finding is, of course, consistent with the well-known fact that in absolute terms unemployment differs more by race among teenagers than among adults.

The effect of an extra year of elementary or secondary education is 0.234 for men 14-24 vs 0.073 for men 25-64. So far as I know, no other sample of young men implies returns to elementary or secondary education of this magnitude (compare e.g. Griliches, 1976). I suspect that my result derives from the inclusion of young men who were students during part of 1969 but were no longer students in March, 1970. Almost half the return to elementary and secondary education among 14-24 year olds disappears when we look at weekly instead of annual earnings, while only a sixth of the return disappears for 25-64 year olds. This is consistent with a very high unemployment rate for 14-24 year olds who have not finished high school. Returns to higher education are not significantly greater for men 14-24 than for men 25-64. Returns to all levels of education are lower for men over 65, though the differences are not quite significant for LnEarnings in this specification. They become significant when we look at weekly earnings.

The experience coefficients vary wildly by age group, suggesting that the quadratic specifications is imprecise.

Up to now I have said nothing about the equations for all men 14-99. The education coefficients are very similar to the equations for 25-64 year olds. Even the experience coefficients are fairly similar. The curve is clearly steeper for 14-99 year olds than for 25-64 year olds, but both peak at about the same point. This is largely because 82 percent of all men 14-99 with positive earnings in 1969 were 25-64. But the coefficients for men 25-64 are also quite similar to those for men outside this age range, even though the means and variances differ. Omitting men 14-24 and 65-99 therefore has little impact on coefficients. It does, however, reduce the standard deviation of the residuals. Restricting the sample to 25-64 year olds therefore underestimates the degree of inequality among men 14-99 who have the same education, experience, skin color, and region of birth. The same probably holds for men who are alike with respect to other traits measured in this survey. But while the standard deviation of the residuals is larger for all men than for men 25-64, R^2 is also larger. Thus if we were to assess the explanatory power of our models solely in terms of R^2, our use of a 25-64 year old sample would lead to an underestimate of the model's power for men 14-99.

4. Background Restrictions

Unlike many investigators, we included both **non-whites and men** with farm fathers in our principal analyses. Table 14.6 shows the regressions of Occupation and Ln Income on Father's Education, Father's Occupation, and Education for OCG 25-64 year olds in the experienced civilian labor force who reported non-zero incomes for 1961. The regressions are all restricted to men with complete data.[4] The regressions show the effects of eliminating non-whites and men with farm fathers, **as others have often done.**

Eliminating non-whites makes very little difference in the Occupation equations. In the Ln Income equations, White, Non-Farm Father, Father's Occupation, Father's Education, and respondent's Education explain 21.3 percent of the variance. Dropping

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[4] Men who failed to report their race, education, or income received an allocated value from CPS, so there is no missing data on **these items. I excluded all men** who failed to report Father's Education or Father's Occupation **even if they had no father at home, in order to maintain comparability with other investigators' results. This makes Table 14.6 slightly different from other OCG tables in this volume.**

Table 14.6

Regressions of Occupation and Income on Background and Education for Men 25-64 in the Experienced Civilian Labor Force, with Non-Zero Income in 1961 and with Complete Data on Father's Education, Father's Occupation, and Occupation.[1/]

Independent Variable		Occupation Regressions				Ln Income Regressions			
		All (N=10,892)		Whites (N=10,000)	White Non-Farm Father (N=7161)	All (N=10,892)		Whites (N=10,000)	White Non-Farm Father (N=7161)
Father's Education	B	[.015]	[.040]	-.009	[.042]	[.003]	[.004]	.001	[-.001]
	beta	[.002]	[.006]	-.001	[.007]	[.014]	[.002]	.004	[-.007]
	r	.325	.325	.304	.284	.222	.222	.189	.147
Father's Occupation	B	.186	.209	.191	.136	.002	.003	.004	.003
	beta	.157	.177	.165	.125	.061	.010	.111	.090
	r	.411	.411	.396	.358	.261	.261	.247	.204
White	B	8.623	-	NA	NA	.558	-	NA	NA
	beta	.094	-	NA	NA	.189	-	NA	NA
	r	.222	.222	NA	NA	.264	.264	NA	NA
Non-Farm Father	B	3.486	-	-	NA	.210	-	-	NA
	beta	.057	-	-	NA	.106	-	-	NA
	r	.219	.219	.224	NA	.200	.200	.194	NA
Education	B	3.492	3.637	3.779	4.092	.068	.077	.068	.058
	beta	.510	.531	.537	.554	.306	.347	.311	.284
	r	.611	.611	.608	.609	.400	.400	.361	.318
S.D. of Residuals		19.184	19.363	19.368	19.452	.721	.740	.711	.654
R^2		.411	.400	.391	.384	.213	.170	.141	.107

[1/] CPS allocated missing values on race, education, and income.

[Coefficients in brackets are less than twice their standard error.]

White and Non-Farm Fathers from the equation reduces R^2 to 0.170. Dropping non-whites from the sample then reduces R^2 to 0.141. Most of this reduction is attributable to the fact that the correlation between Education and LnIncome is lower for whites than for the full sample.

Eliminating men with fathers in agriculture lowers the correlation between father's occupational status and respondent's occupational status from 0.396 to 0.358 but has little overall impact on the equation for occupational status. Eliminating men **with farm** fathers has a more dramatic effect on the regressions involving LnIncome. The correlations of LnIncome with background and education measures all fall by at least a sixth. R^2 falls from 0.141 to 0.107.

Table 14.6 helps explain why we obtained higher values of R^2 than Duncan, Featherman, and Duncan (1972) or Jencks et al (1972). They all looked at white, non-farm OCG men. Expanding their sample to include non-whites and men with farm fathers does not appreciably alter the results for occupational status, but it doubles the percentage of variance we can explain in LnIncome. But **as noted earlier,** R^2 is a deceptive standard for comparing samples with different variances. Despite the increase in R^2, the standard deviation of the residuals is larger for our usual sample than for a sample restricted to whites with non-farm fathers. This also holds when we use Income rather than LnIncome as the dependent variable and divide the residuals by the mean. Inequality among men who are alike on race, farm background, father's education, father's occupational status, and their own education is therefore slightly larger when one looks at all males than when one looks only at white non-farm males.

5. Men with No Data

The Census Bureau reports that it fails to locate about 2 percent of all 25-64 year old males living in the U.S. Of those whom it does locate more than 99 percent provide at least some data. Most surveys do far less well. In OCG, for example, only 83 percent of those originally contacted by CPS returned the mailback questionnaire, even after repeated followups. In the PSID, only 76 percent of the initial sample was located and agreed to be interviewed, and by the time of the fifth interview, on which we based most of our analyses, only 55 percent of the original sample remained. The figures for other surveys are similar, with Census Bureau surveys generally eliciting more cooperation than private surveys.

In order to compensate for non-response, survey organizations weight different groups of respondents according to their estimated response rates. Thus if 80 percent of whites but only 60 percent of blacks participate in the survey, each white participant gets a weight of 1/0.80 = 1.25, while each black gets 1/0.60 = 1.67. Unfortunately, there is no way of knowing all the characteristics that affect a respondent's chance of participating. If one had such data, the survey would not be needed in the first place. One can, however, identify certain gross demographic characteristics that affect response rates, like place of residence, sex, age, race, education, and income. If one believes Census estimates of the distribution of these characteristics for the target population, one can weight one's sample to produce comparable distributions. This may or may not suffice to make the sample distributions of other subtler traits approximate the distributions in the target population.

Weighting a sample to reproduce the population means and variances may not be either necessary or sufficient to reproduce the population covariances, and hence the population correlations and regression coefficients. All we can say is that as the response rate falls, the likelihood that weighting will compensate adequately for non-response also falls. The reader should therefore be somewhat more suspicious of results obtained from samples with low overall response rates, such as PSID, than of results from samples with high response rates, such as the Census and OCG. This is particularly true if, as is the case with PSID, the weighting scheme does not even appear to reproduce the distributions of basic demographic variables altogether accurately.[6]

Weighting has one further effect that also deserves brief comment. Weighting some individuals more heavily than others usually inflates the standard errors of one's estimates. But the relationship is not simple and cannot be estimated accurately simply by knowing the distribution of the weights. One can get rough estimates of the magnitude of the bias by drawing a large number of weighted subsamples from the full sample and comparing the variance of statistics from these subsamples to the variance one would anticipate in an unweighted sample.

The ratio of weighted to unweighted sampling variances gives the "efficiency" of the sampling design. A weighted sample of 1000 with an efficiency of 0.50, for example, has sampling variances comparable to an unweighted sample of 500.

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[6] These issues are discussed in more detail in Chapters 7, 15, and 16 and in Appendix D.

In order to get accurate estimates of standard errors in regression analyses, one should standardize the weights so their mean is equal to the efficiency of the sample. But in stratified samples such as ours, the efficiency of the overall sample is not equal to the efficiency of various subsamples. Precision therefore implies constant reestimation of efficiency and restandardization of weights. We did not attempt this. Instead, we standardized our weights to 1.00. We then treated differences that were more than twice their standard error as "statistically significant." In an unweighted sample, such differences would be significant at the 0.05 level using a two-tailed test, or at the 0.025 level using a one tailed test. In weighted samples they may only be significant at the 0.10 or 0.20 level. But since the 0.05 level is an arbitrary cutoff point to begin with, the fact that we are often using a somewhat more liberal standard should not cause much concern.

6. Men with Parcial Data

Even when respondents agree to be interviewed or return their questionnaires, they seldom provide complete data. Item non-response of 15 percent is quite common in data of the kind that concerns us.

The Census Bureau usually assigns non-respondents the value reported by the last previous respondent who resembles the non-respondent on some presumptively relevant set of traits, such as sex, race, age, and the like. This is a good strategy if one's aim is to reproduce population distribution of single variables. If one uses all the available data to allocate missing values, and if non-respondents are in fact like respondents with similar measured characteristics, it will also reproduce the bivariate distributions. But in samples with large numbers of variables it is often impossible to find another sample member who resembles the non-respondent in every respect. Not even the Census Bureau tries to do this. If this is not done, correlations and regression coefficients will usually be biased downward.

A second common strategy is to compute every statistic for all individuals reporting the necessary data and then to assume that these individuals are representative of the entire sample. If this assumption is correct, one can treat all the observed means, standard deviations, and correlations as if they applied to the full sample and can use them to compute regression equations for the full sample. If the assumption is incorrect, however, one can easily get results that do not apply to any population. If, for example, poor people fail to report their occupations, perhaps because they have trouble describing their work comprehensibly, while rich people fail to report their incomes, a "pairwise present" correlation matrix involving education, occupation, and income will end up using some data for the rich, some data for the poor, and some data for both. The results are unpredictable. In some cases one can get complete nonsense.

A third strategy, which appears preferable in almost every respect to the second, is to use only individuals with complete data. These individuals constitute the "listwise" sample in SPSS, which was the statistical package we used for most of our analyses. The listwise sample typically excludes something like a third of the initial respondents, so it is important to check each of the listwise statistics against the analogous pairwise statistic to see if they differ to any appreciable degree. If they differ, we know that the complete data sample is unrepresentative. It does not follow, however, that we are then better off analyzing the "pairwise" sample. The pairwise sample will, of course, have the apparent advantage of being larger, since each statistic is computed from a larger number of cases. But since multivariate statistics from the pairwise sample involve pooling a large number of overlapping samples, we cannot compute their sampling errors in the usual way even if we assume that item non-response is random. If non-response is not random, the pairwise results have no clear interpretation.

Fortunately, the pairwise and listwise samples yield essentially the same results in all our major samples. The Veterans sample was the main exception. There, missing AFQT scores were non-randomly distributed. The pairwise sample therefore misestimated the change in the coefficient of education when predicting earnings. We therefore used the listwise sample in all our multivariate analyses.

Chapter 15

DO DIFFERENT SURVEYS YIELD SIMILAR RESULTS ?

by

Gregory Jackson

Chapter 1 described our overall target population: non-student, non-military males aged 25-64 with positive earnings. The Veterans, NLS, Talent, and Kalamazoo samples cover different subsets from this target population, restricted by age, geographic location, or military experience. The chapters describing these samples all attempt to assess the impact of these restrictions and I will consider them no further.

The OCG, PA, Census, PSID and NORC Brothers surveys purport to represent the target population. The NORC Brothers sample is so small, however, that even large differences between it and national norms can be due to sampling error. I will therefore ignore it. There are some minor differences among the other four samples. The Census, for example, covers military families. The samples also span the eleven years from 1962 to 1972. There are also differences in the way different organizations drew their samples, in the survey instruments, in the definition of certain concepts, and in the coding of variables in the data files we obtained. It is possible to eliminate many of these differences, but in order to do this one must deliberately reduce the data to crude form, wasting much of its potential value. We therefore based our substantive analyses on samples and coding procedures we know to be somewhat dissimilar, in order to extract maximum information from the data. But it is important to know whether the surveys yield similar results when known differences are eliminated. I eliminated as many differences among these five surveys as I could, first by making sample coverage as similar as possible and second by making variable definitions and coding as similar as possible. I can thus investigate how certain basic relationships differ from sample to sample. If the differences are small, then our diverse samples probably represent a common population. If they are not, then we must be very

Chapter 16 shows precisely how changes in sample coverage and measurement affect the apparent relationship between education and earnings. It is thus complementary to the present analysis, although it is confined to only two variables.

My analysis tries to define the five samples as similarly as possible. Most other studies for this project eliminated student and military respondents. However, this was impossible for the OCG sample. Moreover, the PSID and PA surveys were restricted to heads of households. I therefore kept students and military men in and eliminated men who were not household heads from all five samples. I also excluded respondents lacking data on any of the variables under consideration (except for men who had no father at home at age 15 and failed to report his education or occupation). This has different effects in different samples. OCG respondents who failed to report education or income were allocated values using standard CPS procedures. There were no flags on the OCG tape for identifying allocated values, so OCG respondents who had complete data on other items remain in my sample. Census respondents who failed to report an item also received allocated values. The flag identifying allocated income values was defective in the tape I used, so men who reported all items other than income remain in the Census sample. PSID men who failed to report education but were illiterate were allocated to the lowest education category. These men could not be excluded.

The resulting samples thus consist of male household heads, aged 25 to 64, with positive income and complete data on the relevant variables. This leaves 10,770 men in my OCG sample, 1,223 men in the PA, 33,738 in the Census and 2,301 in the PSID.

Variables

I used nineteen variables for these sample comparisons. In several cases, I sacrificed detail to insure comparable coding for all surveys. The surveys' reports of Father Absent, Father White Collar, Father Foreign, Race, Age, Nonsouth Upbringing, and Nonfarm Upbringing were comparable already, and required no sacrifice or modification.

The PA and PSID did not code Father's Occupation or Occupation into detailed categories. I therefore coded occupational data from all surveys into nine broad categories. I then assigned each category its estimated mean Duncan score. (The actual values are given in Table 15.1.) If an OCG respondent had no father at home when he was 15 or 16 years old and failed to report Father's Occupation, I assigned him the sample mean.

The PA and PSID also used different categories from the Census and OCG to code Father's Education and Education. I collapsed categories to make all the surveys alike.[1]

When a respondent reported fewer than nine years of education for himself or his father, I assigned him a value of 6.5. I assigned mean Father's Education values to nonrespondents who had no fathers at home at age 15 or 16.

The only adjustment required for Siblings was to code any respondent who had more than eight siblings as though he had reported ten.

Income and/or Earnings was available in dollars in the PSID and PA surveys, in hundred-dollar intervals in the Census, and in seventeen differently sized categories in the OCG. Moreover, the mean income of Americans almost doubled from 1961 to 1973, according to the Current Population Survey.

To complicate matters further, a variable giving earnings as distinct from total income was unavailable in the OCG.

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[1] After preliminary analysis, I realized that in fact the Education and Father's Education codings were slightly different. Specifically, only PSID and PA respondents who completed advanced degrees were coded 18, while in the OCG and Census any schooling beyond sixteen years was so coded. This error, which I could not correct in time, probably inflates the estimates of advanced schooling's effects on occupational and financial success for the PSID and PA surveys.

Table 15.1

Frequencies, Means and Standard Deviations of Specially Defined Variables for Subsamples of Four National Surveys

Variable Description	Coding	OCG	PA	Census	PSID
N		10770	1223	33738	2301
White	0	8.7	9.5	8.4	11.0
	1	91.3	90.5	91.6	89.0
	Mean	.913	.905	.916	.890
	S.D.	.282	.293	.277	.313
Father Absent At Age 15/16	0	84.8	(NA)	(NA)	98.9
	1	15.2			1.1
	Mean	.152			.011
	S.D.	.359			.104
Father's Occupation					
Laborer	12	13.0	(NA)	(NA)	9.7
Farmer	14	26.3			26.9
Operative	18	14.1			16.2
Craftsman	31	17.7			22.2
Clerical, Sales	47	7.5			5.2
Self-Employed Business	48	7.5			7.3
Manager	58	3.8			4.9
Professional	75	4.7			6.6
Father Absent & NA[1]	*	5.5			1.1
	Mean	27.590			28.739
	S.D.	17.172			18.245
Father's Education					
8 or less	6.5	66.3	75.6	(NA)	68.2
Some high school	10	9.5	6.2		7.2
Finished high school	12	12.7	10.2		13.6
Some college	14	4.2	3.7		4.4
Finished College	16	2.7	3.4		3.7
Some Graduate Education	18	1.7	0.8		1.8
Father Absent & NA[2]	*	3.0	-		1.1
	Mean	8.351	7.976		8.411
	S.D.	2.951	2.816		3.103
Father White Collar	0	71.0	(NA)	(NA)	75.0
	1	23.5			23.9
Father Absent & NA[3]	*	5.5			1.1
	Mean	.248			.239
	S.D.	.420			.427
Father Foreign	0	75.7	80.9	(NA)[4]	84.2
	1	24.3	19.1		15.5
	Mean	.243	.191		.155
	S.D.	.429	.394		.362

Table 15.1 continued

Variable Description	Coding	OCG	PA	Census	PSID
Siblings	0	5.8	7.4	(NA)	6.2
	1	12.2	14.8		16.2
	2	15.0	17.3		15.8
	3	13.8	12.4		15.3
	4	11.1	10.3		12.2
	5	9.5	10.1		8.6
	6	7.8	7.8		7.0
	7	6.6	5.6		6.0
	10	18.4	14.2		12.7
	Mean	4.515	4.065		3.967
	S.D.	3.179	3.062		2.955
Non-South Upbringing	0	29.4	31.9	(NA)[4]	31.0
	1	70.7	68.1		69.0
	Mean	.707	.681		.690
	S.D.	.455	.466		.462
Non Farm Upbringing	0	21.4	37.9	(NA)	31.7
	1	78.6	62.1		68.3
	Mean	.786	.621		.683
	S.D.	.410	.485		.465
Age (grouped)	25-29	12.0	12.3	13.2	16.7
	30-	14.2	12.1	12.5	12.7
	35-	15.6	13.2	12.7	12.2
	40-	14.9	14.6	13.7	15.5
	45-	13.4	14.6	14.1	13.7
	50-	12.6	12.4	12.8	11.8
	55-	10.0	11.0	11.5	9.2
	60-64	7.5	9.6	9.4	8.2
(ungrouped)	Mean	42.899	43.769	43.697	42.481
	S.D.	10.619	11.057	11.114	11.137
Education	6.5	27.8	24.2	21.4	18.0
	10	19.1	19.1	19.4	16.2
	12	28.6	29.9	31.3	30.9
	14	10.4	11.5	11.9	16.5
	16	8.2	11.7	7.9	11.9
	18	6.0	3.6	8.1	6.4
	Mean	10.988	11.202	11.375	11.878
	S.D.	3.470	3.329	3.571	3.312
Experience (grouped)	0	0.1		0.2	0.1
	1-5	2.7	3.4	3.4	5.1
	-10	9.9	10.4	11.1	13.1
	-15	13.3	10.9	12.1	13.2
	-20	14.1	13.1	11.9	11.4
	-25	14.1	13.6	12.2	13.6
	-30	12.7	13.1	13.3	13.5
	-35	11.4	11.6	11.9	10.6
	-40	9.9	9.9	10.6	8.6
	-45	7.5	8.3	8.4	7.0
	-50	4.3	5.8	4.8	3.9
(ungrouped)	Mean	24.772	25.446	25.108	23.513
	S.D.	11.859	12.354	12.340	12.221

Table 15.1 continued

Variable Description	Coding	OCG	PA	Census	PSID
Occupation					
Laborer	12	12.4	9.4	12.3	8.9
Farmers	14	5.9	5.8	2.9	3.8
Operatives	18	18.9	15.5	18.8	16.4
Misc.	29		3.5		
Craftsmen	31	21.2	21.9	23.6	21.5
Clerical, Sales	47	11.6	11.1	13.7	11.5
Self-Employed	48	7.5	9.5	3.5	7.3
Managers	58	9.2	9.4	9.4	13.7
Professionals	75	13.5	13.8	15.7	16.9
Mean	Mean	36.745	38.147	37.937	40.752
	S.D.	20.986	20.453	21.310	21.298
Income	250	2.6	0.8	1.4	0.5
	750	2.0	1.7	1.8	0.9
	1250	2.4	2.0	2.3	1.9
	1750	2.4	2.5	3.0	2.0
	2250	3.7	2.4	2.4	3.2
	2750	3.7	3.9	4.1	4.2
	3250	5.7	3.4	5.5	3.8
	3750	5.1	4.8	6.4	4.5
	4250	6.5	7.5	4.7	6.1
	4750	6.7	5.9	8.9	6.6
	5500	16.2	13.4	16.6	13.2
	6500	13.0	13.1	12.4	12.6
	7500	10.3	11.4	9.4	11.0
	9000	8.8	13.0	9.3	14.4
	12000	7.4	8.8	7.5	10.2
	18000	2.8	3.3	2.9	3.7
	33000	1.0	2.0	1.3	1.2
	Mean	6250	7049	6436	7066
	S.D.	4397	5082	4626	4601
Ln Income	Mean	8.499	8.646	8.549	8.678
	S.D.	.792	.702	.726	.646
Income 1/3	Mean	17.535	18.312	17.752	18.435
	S.D.	4.047	4.010	3.935	3.753

1/ PSID did not ask this question. We treated fathers as absent if sons reported neither Father's Education nor Father's Occupation. OCG nonrespondents were assigned 27.1. PSID nonrespondents were assigned 25.

2/ PSID did not ask this question. We treated fathers as absent if sons reported neither Father's Education nor Father's Occupation. OCG respondents were assigned 8.4. PSID respondents were assigned 8.

3/ See footnote 2. OCG respondents were assigned 0.237. PSID respondents were assigned 0.25.

4/ The Census asked this question, but it was not available on my extract tape.

These complications dictated a three-step adjustment procedure. First, I used total income rather than earnings in all surveys. Next, I deflated income to 1961 levels, using the ratio of CPS mean income for 25-64 year old men in the year under study to CPS mean income in 1961 as a divisor. Finally, I assigned the respondent the midpoint, in dollars, of the OCG category into which this deflated total fell (see Table 15.1).

When Are Samples "Similar"?

The basic criterion for sample similarity is simple: it must be reasonable to assert that the samples are drawn from the same population. This is impossible to determine directly, since the characteristics of the population are unknown. The usual procedure is to use data from different samples to infer a set of population parameters and then to see whether the distribution of sample estimates accords with statistical predictions. However, even when deviations from such ideal distributions are significant, as they almost always are for the two largest samples in my analysis, they are often too small to deserve serious attention. Rather than rely on statistical tests of significance, therefore, I will present the actual parameters, along with enough statistics for the reader to make such tests as he or she may desire.

The bulk of the analysis in this report involves multiple-regression equations. Regression results depend on sample means, standard deviations and correlations. The next section of this chapter discusses the variables' frequency distributions, which determine their means and standard deviations. After that I examine bivariate relationships, both by comparing regression coefficients and by considering changes in the mean and standard deviation of the outcome variables -- Education, Occupation, and Income -- across categories or specified ranges of the background variables.

This set of analyses will permit me both to evaluate the general hypothesis that these four samples represent the same population and to identify differences in some detail. Since I have chosen not to use formal statistical tests to examine the differences, I must specify some alternative criteria for similarity. For frequency distributions, three characteristics are important: general shape, the presence of outliers or distinct features, and central tendency. (I consider dispersion to be part of shape.) The sample-to-sample variation in the first two can be assessed from a simple frequency table, that of the last from the mean, median, and/or mode of the distribution. For bivariate relationships, there are also three important characteristics: whether the relationship is linear, the slope of the regression line, and the amount of variation around the regression line. Differences in these characteristics can be assessed from the tables which give the mean and standard deviation of a "dependent" or outcome variable for each category or range of an "independent" or background variable.

The tables in the next two sections include enough information to perform standard statistical tests. In general, however, I will compare variables' distributions and interrelationships using graphic methods, and will comment on those features that make particular graphs very different.

Frequency Distributions

Table 15.1 presents frequencies, means, and standard deviations for each variable by sample. It is important in examining these statistics to remember that the data span ten years, and that only Income is adjusted for changes over this period. With that warning in mind, I will proceed to consider them *seriatim*.

The PSID sample has the highest proportion of non-white respondents, 11 percent, while the Census has the lowest, 8.4 percent. The difference between PSID and the other surveys derives from the inclusion of Spanish Americans as non-whites - a regrettable error. The other differences are too small to deserve analysis.

The mean Duncan scores for Father's Occupation in the OCG and the PSID differ by 1.1 points. An examination of the frequencies suggests that this difference arises in the extremes of the distribution: there are 3.3 percent fewer laborers and service workers (Duncan score 12), 1.1 percent more managers (58), and 1.9 percent more professionals (75) in the PSID than there are in the OCG. Again, given the 10 year interval between surveys, the differences appear reasonable and minor.

The distributions of Father's Education are remarkably similar for the OCG, PA, and PSID samples. The PA mean is one-half year lower than the others, primarily because about 8 percent more respondents in this survey had fathers with less than eight years of schooling. Since two thirds or more of the responses are grouped in this lower category, it is difficult to say whether the differences across the four samples on this variable are consistent with any particular explanation.

The proportion of respondents whose fathers were born outside the U.S. varies widely, from 24.9 percent in the OCG to 15.5 percent in the

PSID. This presumably reflects the decline in immigration after 1914.

The data for Siblings reflect the American trend toward smaller families. The mean declines from the OCG's 4.5 to the PSID's 4.0; the incidence of small families increases while that of large families declines.

The proportion of respondents born or raised in the South does not vary much from survey to survey. This is not true for farm upbringing: the 21.4 percent of the OCG respondents who were raised on a farm is 10 percentage points less that the PSID's 31.7 percent and a full 16 percentage points lower than the PA's 37.9 percent. This probably results from differences in the questions used to obtain these data. PSID and PA respondents were all asked whether they had grown up on a farm. OCG respondents were asked whether they were still living in the same community as when they were 16. If they had moved, they were then specifically asked if they had lived on a farm at 16. If they had not changed communities, they were only assumed to have lived on a farm at 16 if they lived on a farm at the time of the survey. Those who had moved from a farm to a town in the same "community", or whose "community" had evolved from farm into town were thus treated as having grown up in a town.

The Age and Education distributions do not vary markedly from survey to survey, except to reflect the continuing increase in average educational attainment. There are some differences, most notably the 8.1 percent of the Census respondents who reported more than sixteen years of schooling. This is probably due, as I noted before, to the Census' willingness to count years of schooling beyond 16 even if no degree ensued. The Experience variable in each survey is constructed from Age and Education, and is used primarily to reduce the bias in multiple-regression estimates of Education's effects. Therefore, its distribution is completely determined by those variables' distributions and interrelationship, and I will not analyze it in any detail.

As the U.S. has moved from a labor-bound to a service- and supervision-oriented economy, the distribution of Occupation should have changed. Since service and supervisory occupations have a higher status in Duncan's scheme than labor occupations, the average Duncan score should have risen. This trend is evident in the four surveys I am comparing. However, they also display some systematic variation that is not consistent with this trend. Specifically, the PA and PSID respondents, who were selected and interviewed by the University of Michigan's Survey Research Center, have a higher average Duncan score than the OCG and Census respondents, who were selected and interviewed by the Census Bureau. The source of these differences is apparent in the distributions themselves: the SRC seems to find and/or successfully interview fewer laborers and operatives and more self-employed businessmen than the Census Bureau does. I have neither a ready explanation for these findings nor any estimate of their likely impact.

It is worth noting that neither PA nor PSID respondents are more likely than OCG respondents to be farmers, which reinforces my view that the disparities among samples in Nonfarm Upbringing are due to the questions used and not to sample differences.

The differences between SRC and Census Bureau samples reappear in the Income statistics, which are adjusted to 1961 levels. The PA and PSID means are virtually identical, and six to eight hundred 1961 dollars higher than the OCG and Census means. The distributions reflect these differences: there are fewer poor and more well-off respondents in the PA and PSID samples than there are in the Census and OCG samples.

It is clear from this review of simple frequency distributions that the four samples I am comparing do not describe the same population. Instead, they describe populations that differ in two systematic ways, over time and by survey organization. The first of these is to be expected, and need cause no worry, but the second is more troubling.

The basic difference between the populations sampled by the Survey Research Center and the Census Bureau is their socioeconomic status, whether measured by mean Duncan score, the size of different occupational categories, or mean income. Relative to the Census sampling frame, the SRC tends to successfully interview either fewer poor, low-status respondents or more well-off, high-status ones than it should.[2]

However, its respondents' race, family size, and upbringing do not differ from those of Census Brueau respondents, so the bias must be accomplished in subtle ways.

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[2] I have intentionally implied that the Census frame is more realistic. Most evaluations of the Census Bureau's surveys contend that low-status persons are underenumerated. Therefore, if both organizations err, the SRC's error is in the same direction as and larger than the Census'.

Bivariate Relationships

Only a two-way joint frequency distribution fully describes the relationship between two variables. Unfortunately, displaying these distributions takes a great deal of space, and the amount of detail they contain makes them very difficult to interpret, let alone compare. In this analysis I will not examine joint frequency distributions but summary tables giving the mean and standard deviation of Education, Occupation, and Income for each category of each causally prior variable. Such tables can be used to construct regression equations and assess their explanatory power, but I will not emphasize this application. My interest is whether the relationship of outcome to background variables varies from survey to survey. These tables provide a clearer answer than regression coefficients do. The progression of means reflects the linearity or nonlinearity of the relationship, while the pattern of standard deviations indicates how coherent -- that is, grouped around the average trend -- the relationship is. From this information it is possible to infer how accurately the bivariate correlation and regression coefficients, which both assume linearity, summarize the relationships. Tables 15.2 through 15.16 give means and standard deviations of each outcome variable by categories of each background variable and the corresponding correlation and regression coefficients. Plots of outcome means by background-variable categories accompany most of the tables, and are numbered correspondingly. I will start by looking at the impact of a respondent's traits on his education. Then I will look at these same traits' impact on his occupation. Finally, I will look at their impact on his income.

The Determinants of Education

Figure 15.2 displays the progression of Education means for the OCG and PSID samples (the data are from Table 15.2). The two patterns are similar except the PSID is generally higher, and particularly so at the lower end (laborers, service workers, farmers, and operatives). Since almost half of each survey's respondents fall in this lower range, the best-fitting regression lines have different slopes: a ten-point difference in father's Duncan score is associated with an 0.85 year increase in Education in OCG and an 0.67 year increase in PSID. There are also differences in the variation around this line: the PSID men with low status fathers have more varied education than their OCG counterparts, while PSID men with high status fathers are less varied than their OCG counterparts.

The apparent effects of Father's Education in Table 15.3 resemble one another more than those of Father's Occupation. There appears to be a rather uniform, linear relationship between Father's Education and Education. In each survey, however, about three quarters of the respondents' fathers completed eight or fewer years of schooling, and this weights heavily the left or lower part of the plots -- where the surveys differ most -- in least-squares calculations. The regression coefficients in Table 15.3 reflect this. The PSID curve, which rises most slowly on the left side of the plot, yields the smallest coefficient, while the other two surveys' virtually parallel lower plots constrain their coefficients to be almost equal.3/

3/ Differences between little-educated fathers may have a smaller impact on Education in the PSID than they do in other surveys. An alternative explanation, however, is that 6.5 years reasonably characterizes the zero-to-eight category in the OCG and PA samples but is too low for the PSID.

Table 15.2

Education by Categories of Father's Occupation for OCG and PSID Male Household Heads 25-64 With Complete Data and Non-Zero Income

Father's Occupation (Duncan Score)	OCG Mean (S.D.)	OCG N	PSID Mean (S.D.)	PSID N
Total	10.988 (3.470)	10770	11.878 (3.312)	2301
Laborers or Service (12)	10.094 (3.074)	1395	10.278 (3.183)	223
Farmers (14)	9.450 (3.167)	2825	10.672 (3.390)	619
Operatives (18)	10.715 (3.084)	1521	11.806 (3.085)	372
Craftsmen, Foremen (31)	11.219 (3.105)	1908	11.899 (2.821)	510
Clerical, Sales (47)	13.154 (2.941)	811	13.301 (2.732)	120
Self-Employed Business (48)	12.854 (3.432)	803	13.843 (3.202)	167
Managers, Officials (58)	13.863 (2.800)	411	14.267 (1.986)	112
Professional, Teachers (75)	14.502 (3.165)	504	14.383 (2.629)	152
Father Absent *	9.907 (3.098)	593	10.807 (3.142)	25
Correlation (r)	.41895		.37525	
Unstandardized Regression Coefficient (b)	.08466		.06762	

*Question not asked in PSID. Category includes men who failed to answer questions on both father's education and father's occupation.

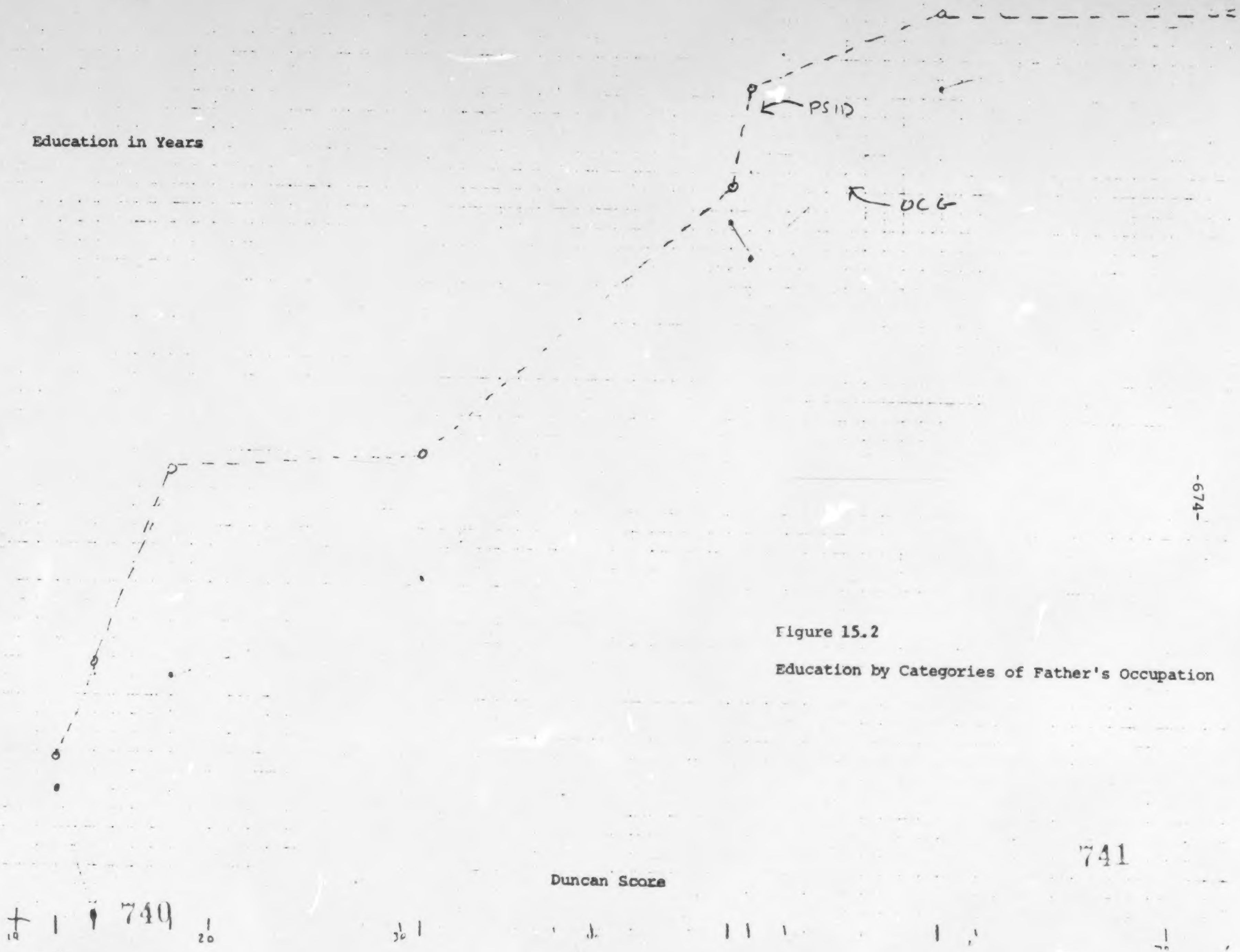

Figure 15.2

Education by Categories of Father's Occupation

Table 15.3

Education by Categories of Father's Education for OCG, PA, and PSID Male Household Heads 25-64 with Complete Data and Non-Zero Income.

Father's Education	OCG		PA		PSID	
	Mean (S.D.)	N	Mean (S.D.)	N	Mean (S.D.)	N
Total	10.991 (3.469)	10764	11.202 (3.329)	1223	11.878 (3.312)	2300
0-8 (6.5)	10.188 (3.274)	7130	10.416 (3.146)	925	11.104 (3.247)	1570
9-11 (10)	12.011 (3.055)	1024	12.789 (2.459)	76	12.886 (2.884)	165
12	12.663 (3.055)	1363	13.304 (2.701)	125	13.334 (2.687)	312
13-15 (14)	13.390 (2.932)	447	14.411 (2.507)	45	14.178 (2.384)	101
16	14.448 (2.886)	292	14.905 (2.128)	42	14.405 (2.521)	86
17 or more (18)	15.673 (2.594)	183	15.600 (1.838)	10	15.977 (1.902)	42
Father Absent *	9.367 (3.005)	326			10.807 (3.142)	25
r	.38739		.43062		.37306	
b	.45552		.50907		.39819	

*Question not asked in PSID. Category includes men who failed to answer questions on both father's education and father's occupation.

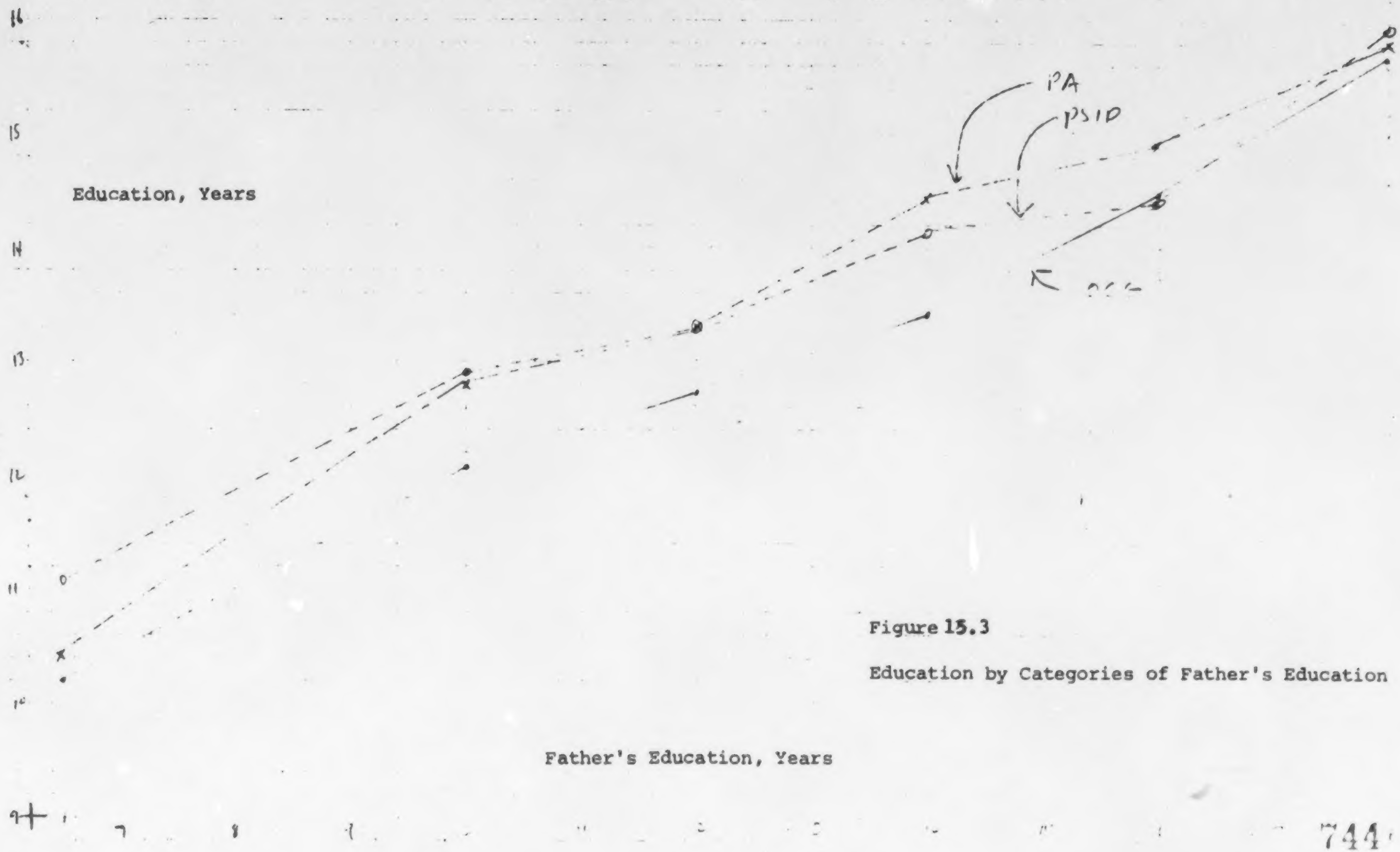

Figure 15.3

Education by Categories of Father's Education

The different surveys' correlations between Education and Siblings, in Table 15.4, cluster around -0.32. The regression coefficients are also similar,/ and figure 15.4 demonstrates why this is so: the OCG and PA patterns are virtually identical and parallel to the PSID pattern, which is about 0.7 years higher.

Figure 15.5 presents the data from Table / 15.5, but with Age translated into year of birth. Since Education generally increases little past the and 35 men who were, say, 35 to 39 years old in the 1962 OCG survey should have the same mean Education as those who were 45 to 49 years old in the 1972 PSID survey. The plots support this view very well for men born after 1925. They support it reasonably well for older men, and there is no clear pattern to the departures.

The Determinants of Occupation

Figure 15.2 and Table 15.2 showed that men with low-status fathers got more education in the PSID than in the OCG. It is reasonable to expect these better-educated men to have entered higher status occupations, and the data in figure and Table 15.6 fulfill this expectation. The 152 men in the PSID sample whose fathers' occupations were professional or technical got about the same Education as their OCG counterparts, but their occupational status was 6 Duncan points -- or about two sevenths of a standard deviation -- lower. In general, both the table and the plots suggest the differences in the effect of Father's Occupation on Education carry over into its effects on Occupation.

The sample-to-sample differences in the effect of Father's Education on Education also carry forward into Occupation. Again the regression and correlation coefficients are heavily influenced by the mean for

Table 15.4

Education by Number of Siblings for OCG, PA and PSID Male Household Heads 25-64 with complete Data and Non-Zero Income

	OCG		PA		PSID	
	Mean (S.D.)	N	Mean (S.D.)	N	Mean (S.D.)	N
Total	10.988 (3.470)	10770	11.202 (3.329)	1223	11.878 (3.312)	2301
0	12.578 (3.302)	628	12.555 (3.268)	91	13.342 (2.993)	143
1	12.865 (3.266)	1308	12.680 (3.202)	181	13.187 (2.975)	373
2	12.106 (3.261)	1611	12.012 (3.216)	212	12.867 (3.194)	363
3	11.192 (3.389)	1484	11.638 (2.922)	152	12.367 (3.057)	352
4	10.868 (3.268)	1194	10.913 (2.991)	126	11.479 (3.077)	281
5	10.339 (3.287)	1017	10.460 (2.894)	124	11.237 (3.066)	197
6	9.843 (3.057)	844	9.895 (3.275)	95	10.449 (2.968)	160
7	9.660 (3.190)	707	9.603 (3.243)	68	10.163 (2.882)	139
8+ (10)	9.547 (3.241)	1978	9.667 (3.268)	174	10.092 (3.447)	293
r	-.32552		-.31899		-.33916	
b	-.35532		-.37202		-.38013	

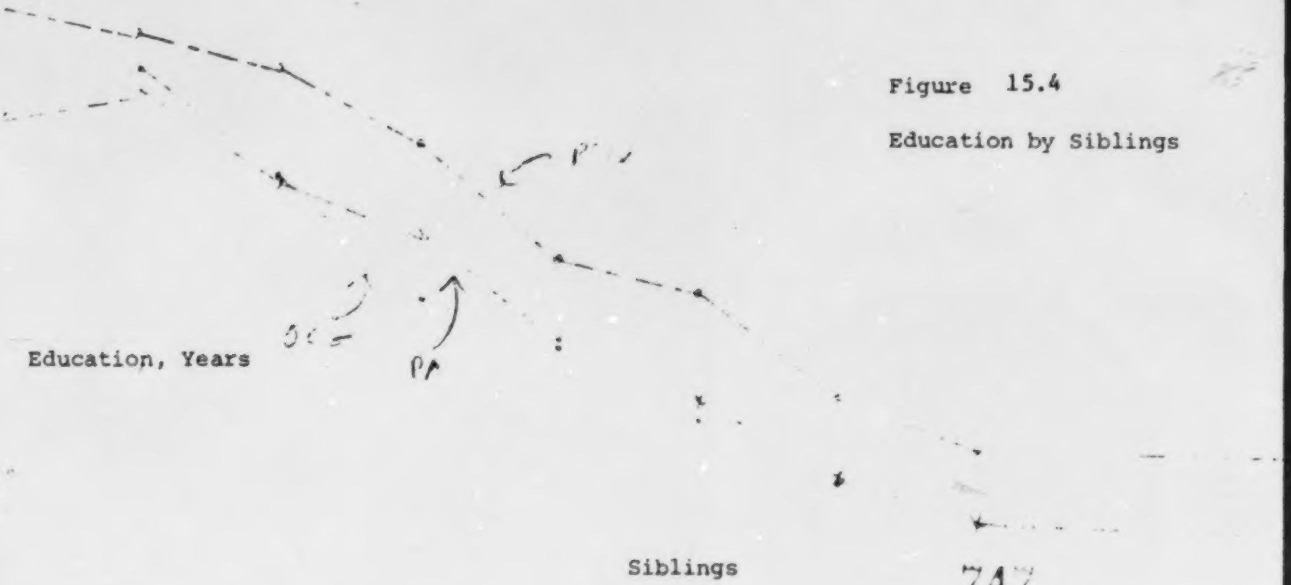

Figure 15.4

Education by Siblings

Table 15.5

Education by Age for OCG, Pa, Census and PSID Male Household Heads 25-64 with Complete Data and Non-Zero Income

Age	OCG (1962)		PA (1964)		Census (1970)		PSID (1972)	
	Mean (S.D.)	N	Mean (S.D.)	N	Mean (S.D.)	N	Mean (S.D.)	N
Total	10.988 (3.470)	10770	11.202 (3.329)	1223	11.482 (3.413)	33738	11.878 (3.312)	2301
25-29	12.039 (3.084)	1289	12.503 (2.829)	151	12.639 (2.842)	4460	12.686 (2.600)	383
30-34	11.915 (3.313)	1527	12.155 (3.343)	148	12.356 (3.124)	4229	12.635 (2.864)	293
35-39	11.518 (3.393)	1679	11.719 (3.064)	162	12.137 (3.366)	4281	12.294 (3.273)	282
40-44	11.146 (3.344)	1603	11.699 (3.052)	178	11.614 (3.412)	4622	12.223 (3.299)	356
45-49	10.768 (3.356)	1441	10.941 (3.219)	179	11.146 (3.346)	4754	11.405 (3.426)	316
50-54	10.316 (3.501)	1358	10.743 (3.386)	152	10.894 (3.356)	4330	11.736 (3.429)	272
55-59	9.698 (3.459)	1070	10.252 (3.420)	135	10.331 (3.458)	3887	10.475 (3.503)	211
60-64	9.360 (3.518)	802	8.953 (3.142)	118	9.885 (3.508)	3175	10.356 (3.642)	188
r	-.24840		-.29513		-.25462		-.22194	
b	-.08117		-.08886		-.08181		-.06600	

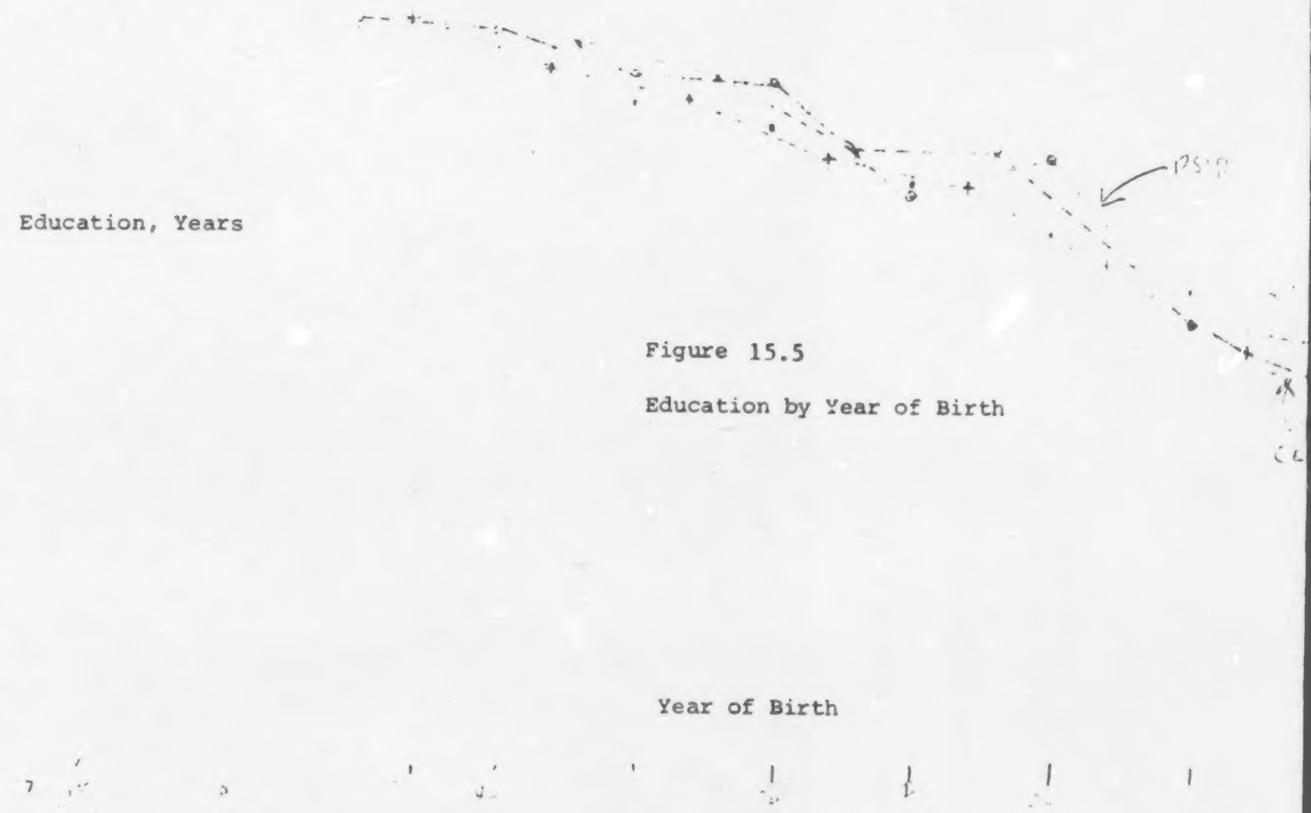

Figure 15.5

Education by Year of Birth

Table 15.6

Occupational Status by Father's Occupation for OCG and PSID Male Household Heads 25-64 with Complete Data and Non-Zero Income

Father's Occupation	OCG		PSID	
	Mean (S.D.)	N	Mean (S.D.)	N
Total	36.745 (20.983)	10770	40.753 (21.298)	2301
Laborer or Service (12)	30.493 (18.815)	1395	32.430 (19.656)	223
Farmer (14)	28.634 (18.042)	2825	34.242 (20.325)	619
Operative (18)	34.888 (20.111)	1521	40.610 (21.581)	372
Misc. (19/29)	-		-	
Craftsmen & Foremen (31)	38.654 (19.662)	1908	41.980 (20.024)	510
Clerical & Sales (47)	48.387 (20.492)	811	48.135 (19.403)	120
Self-Employed (48) Business	48.077 (19.330)	803	51.376 (19.187)	167
Managers, Officials (58)	51.487 (19.439)	411	54.460 (17.775)	112
Professionals, Teachers (75)	55.101 (21.196)	504	49.191 (22.135)	152
Father Absent	31.637 (19.114)	593	34.506 19.661	25
r	.38129		.27637	
b	.46598		.32262	

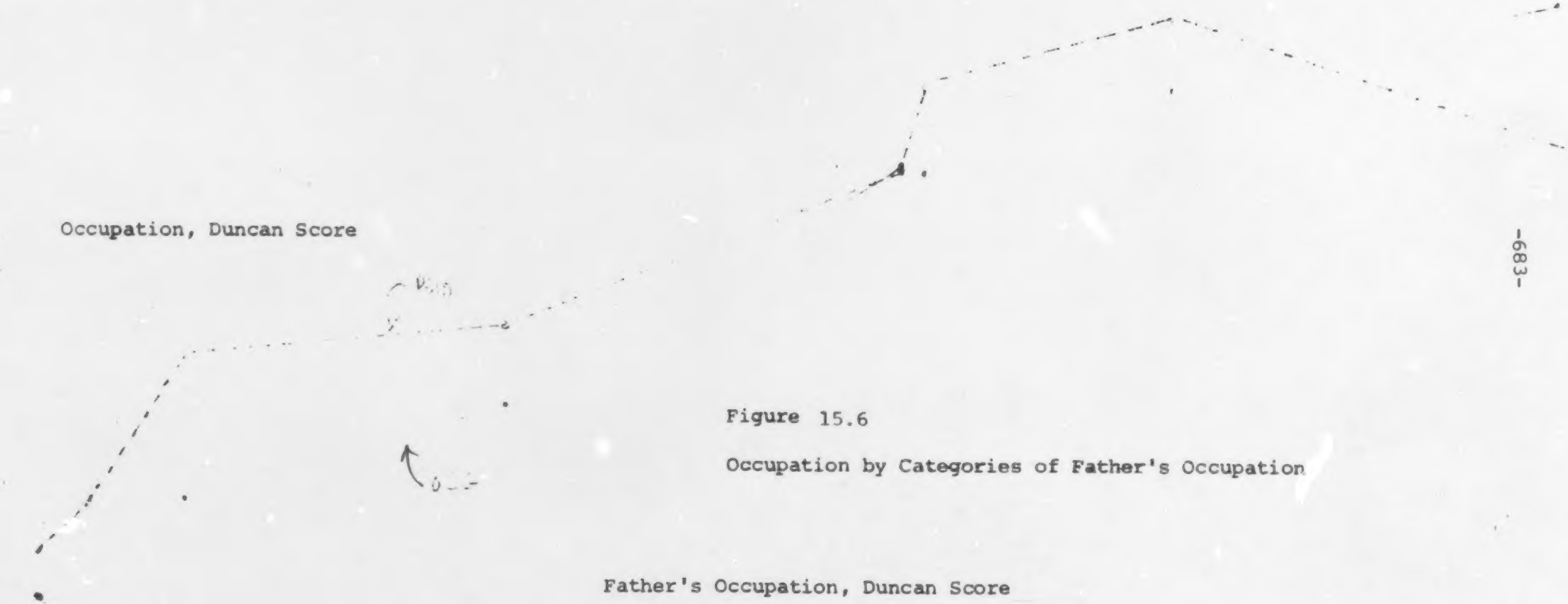

Figure 15.6

Occupation by Categories of Father's Occupation

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respondents with little-educated fathers, but in general the plots in Figure 15.7 are parallel. The 10 PA respondents whose fathers got 17 or more years of schooling had a mean Occupation strikingly lower than their OCG and PSID counterparts, but since there are only 10 of them, their statistical impact is small.

The relationship between Siblings and Occupation in Table 15.8 is quite similar in the different samples, as the near-equal regression and correlation coefficients suggest. The plots support this, except that the 152 PA respondents who had three siblings seem to have had a higher mean than their counterparts in the other surveys. This discrepancy is not due to differences in Education; the three-sibling respondents in all three surveys got about the same amount of schooling (see Table and Figure 15.4.

Duncan's status coding for occupations depends on the education and income that men in each occupational category reported to the 1950 Census. The coincidence of the plots in Figure 15.9 (and of the corresponding data in Table 15.9) indicates that the general distribution of Duncan scores within education categories has remained constant. This, in turn, suggests both that the relationship between education and occupational status has remained fairly constant or, in effect, that the Duncan scale's validity as a measure of educationally-defined occupational status -- at least at this aggregate level -- did not change over the 1961-1971 period.

I did not consider Table 15.10, Occupation by Experience, in any detail because of the dependence of Experience on Age and Education. In any event, there are only minor differences to explain.

Table 15.7

Occupational Status by Father's Education for OCG, PA, and PSID Male Household Heads 25-64 with Complete Data and Non-Zero Income

Father's Education	OCG		PA		PSID	
	Mean (S.D.)	N	Mean (S.D.)	N	Mean (S.D.)	N
Total	36.753 (20.991)	10764	38.147 (20.453)	1223	40.754 (21.293)	2300
0-8	33.106 (19.533)	7130	34.717 (19.325)	925	37.348 (20.583)	1570
9-11	40.104 (21.472)	1024	45.039 (20.389)	76	44.375 (22.289)	165
12	44.356 (21.760)	1363	47.504 (19.996)	125	47.898 (20.362)	312
13-15	48.332 (20.529)	447	52.289 (21.116)	45	50.794 (20.411)	101
16	54.014 (20.493)	292	56.143 (17.092)	42	52.847 (18.360)	86
17 or more (18)	57.166 (19.399)	183	46.900 (23.530)	10	55.659 (21.367)	42
Father Absent	31.414 (18.429)	326			34.506 (19.661)	25
r	.29306		.30154		.25488	
b	2.0841		2.1901		1.7494	

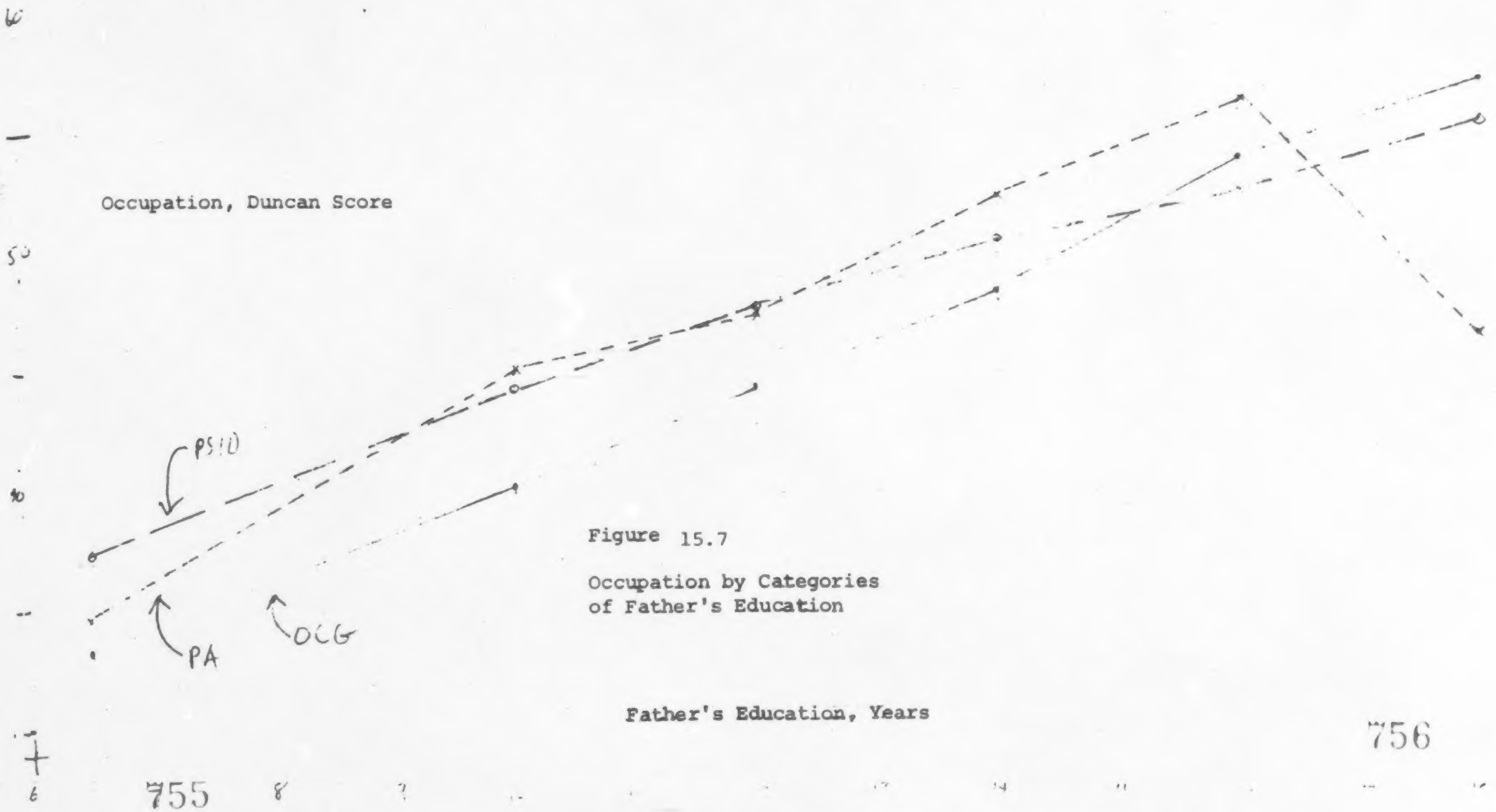


Figure 15.7

Occupation by Categories of Father's Education

Table 15.8

Occupational Status by Number of Siblings for OCG, PA, and PSID Male Household Heads 25-64 with Complete Data and Non-Zero Income

Number of Siblings	OCG Mean (S.D.)	N	PA Mean (S.D.)	N	PSID Mean (S.D.)	N
Total	36.747 20.980	10770	38.147 (20.453)	1223	40.752 (21.299)	2301
0	43.654 (22.236)	628	42.758 (22.162)	91	44.835 (23.761)	143
1	45.155 (22.120)	1308	43.492 (21.158)	181	47.911 (20.583)	373
2	41.176 (21.496)	1611	42.005 (21.142)	212	46.029 (21.533)	363
3	37.390 (20.788)	1484	42.513 (20.270)	152	40.762 (21.464)	352
4	36.497 (19.980)	1194	36.087 (18.445)	126	38.055 (19.755)	281
5	33.589 (19.752)	1017	33.298 (18.796)	124	37.823 (19.960)	197
6	31.573 (18.571)	844	33.274 (18.889)	95	36.573 (20.061)	160
7	31.736 (19.175)	707	32.397 (18.356)	68	33.048 (19.330)	139
8+ (10)	30.679 (18.888)	1978	31.517 (18.502)	174	33.580 (19.554)	293
r	-.22830		-.21560		-.22058	
b	-1.5071		-1.4401		-1.5898	

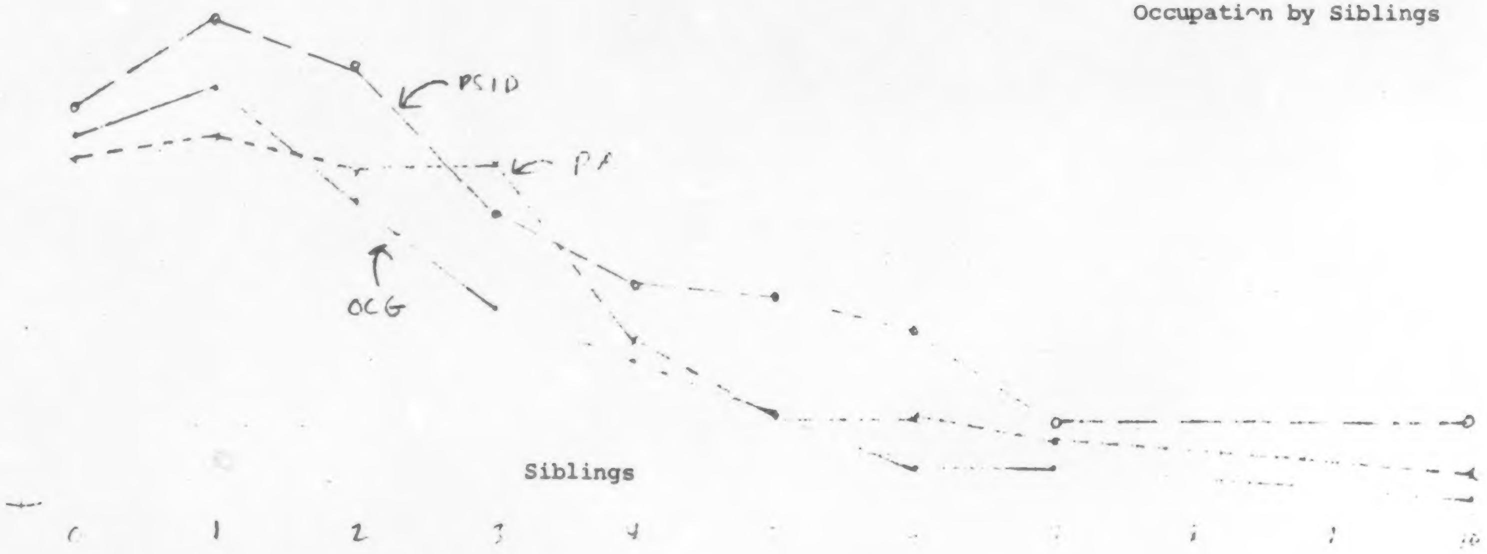


Figure 15.8

Occupation by Siblings

Table 15.9

Occupational Status by Education for OCG, PA, Census, and PSID Male Household Heads 25-64 with Complete Data and Non-Zero Income

Education	OCG		PA		Census P		PSID	
	Mean (S.D.)	N	Mean (S.D.)	N	Mean (S.D.)	N	Mean (S.D.)	N
Total	36.744 (20.986)	10770	38.147 (20.453)	1223	37.937 (21.310)	33738	40.753 (21.297)	2301
0-8	24.289 (13.817)	2992	24.436 (12.737)	296	24.186 (12.929)	7207	25.530 (14.426)	414
9-11	29.271 (15.842)	2051	30.468 (14.538)	233	29.054 (15.513)	6537	30.235 (15.826)	374
12	36.198 (18.151)	3075	37.732 (18.101)	366	35.730 (18.489)	10544	37.128 (17.864)	710
13-15	46.396 (19.345)	1124	48.908 (19.695)	141	47.315 (20.119)	4026	46.874 (18.551)	380
16	61.665 (15.357)	877	59.531 (16.452)	143	60.027 (16.559)	2676	61.813 (15.733)	275
17 and over	69.871 (10.689)	651	70.523 (10.440)	44	68.356 (12.708)	2748	72.569 (6.845)	148
r	.60456		.59748		.60103		.60752	
b	3.6563		3.6708		3.5867		3.9067	

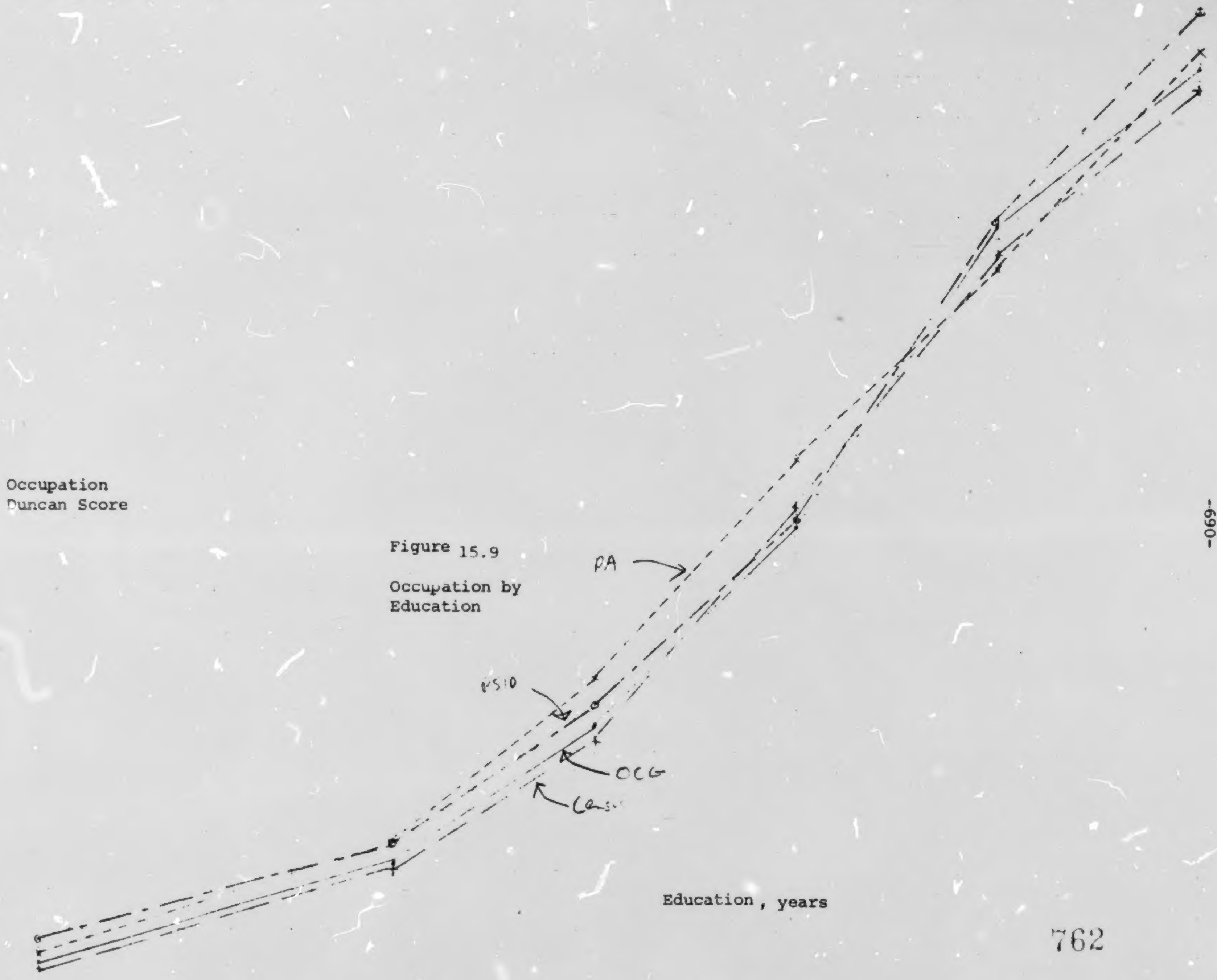


Figure 15.9

Occupation by Education

Table 15.10

Occupational Status by Experience for OCG, PA, Census, and PSID Male Household Heads 25-64 with Complete Data and Non-Zero Income

Experience	OCG Mean (S.D.)	OCG N	PA Mean (S.D.)	PA N	Census Mean (S.D.)	Census N	PSID Mean (S.D.)	PSID N
Total	36.747 (20.982)	10770	38.147 (20.453)	1223	37.937 (21.310)	33738	40.752 (21.299)	2301
0	71.470 (9.623)	12	-		69.507 (16.489)	71	75.000 (0.000)	2
1-5	61.150 (19.790)	289	59.122 (19.568)	41	61.151 (19.293)	1149	54.236 (22.719)	118
6-10	43.165 (23.258)	1066	42.244 (22.890)	127	42.095 (22.473)	3750	43.849 (20.718)	301
11-15	39.267 (22.052)	1430	40.827 (20.144)	133	40.478 (22.107)	4086	43.348 (21.533)	303
16-20	36.605 (20.767)	1520	39.688 (19.766)	160	40.214 (21.679)	4007	42.748 (22.887)	261
21-25	36.307 (19.960)	1522	38.307 (20.939)	166	38.833 (20.966)	4123	42.737 (21.208)	312
26-30	35.445 (20.039)	1366	38.762 (20.048)	160	36.931 (20.380)	4495	40.248 (19.936)	310
31-35	34.295 (18.587)	1231	37.049 (18.944)	142	35.760 (19.903)	4023	35.197 (18.856)	245
36-40	33.696 (19.520	1060	32.620 (18.444)	121	34.050 (19.365)	3583	39.604 (20.626)	197
41-45	30.415 (17.627)	806	32.333 (17.908)	102	30.705 (17.553)	2838	29.385 (16.606)	160
46-50	28.432 (17.586)	468	28.423 (16.269)	71	25.559 (14.397)	1613	29.927 (18.362)	90
r	-.21675		-.23125		-.24672		-.22834	
b	-.38357		-.38288		-.42606		-.39794	

The Determinants of Income

The remaining tables describe the determinants of Income. The average Income in the PA and PSID samples, even when deflated to 1961 levels, was higher than it was in the OCG and Census samples. This general finding reappears in the broken-down statistics; with few exceptions the PA and PSID plots of Income by background characteristics lie above the OCG and Census lines.

From Table 15.6 I found that respondents with high-status fathers had considerably lower-status occupations in the PSID than they did in the OCG, but that otherwise the PSID curve lay above the OCG one. This pattern is repeated, for Income, in Figure and Table 15.11, and it accounts for the difference in the regression and correlation coefficients. The variance for the 411 OCG and 152 PSID respondents with high-status fathers is larger than the average variance, but the pattern does not differ from survey to survey.

The relationship of Occupation to Father's Education (Table/Figure 15.7) was quite similar for the OCG, PA, and PSID samples, except that PA men with well-educated fathers got less schooling than one might expect from the plot or the other surveys. These similarities do not obtain for Income (Figure 15.12); only the PA plot resembles its Occupation counterpart. The association of Income with Father's Education is linear in the PA except for the top two categories. The OCG plot of Occupation by Father's Education was virtually linear; for Income it parallels the PA's decline in the high range. In the PSID sample, mean income rises only slowly with Father's Education, but the 42 men whose fathers got more than sixteen years of schooling had a mean Income of $10,758 -- or $2600 more than the mean for respondents whose fathers got only sixteen years of schooling.

Table 15.11

Income (in 1961 Dollars) by Father's Occupation for OCG and PSID Male Household Heads 25-64 with Complete Data and Non-Zero Income

Father's Occupation	OCG Mean (S.D.)	OCG N	PSID Mean (S.D.)	PSID N
Total	6251 (4397)	10770	7066 (4601)	2301
Laborer and Service (12)	5368 (3284)	1395	6105 (3938)	223
Farmer (14)	4755 (3144)	2825	6248 (4582)	619
Operative (18)	6124 (3444)	1521	6949 (4155)	372
Craftsmen and Foremen (31)	6590 (3616)	1908	7189 (4078)	510
Clerical and Sales (47)	7932 (5689)	811	7709 (5708)	120
Self-employed (48)	8026 (5589)	803	9290 (6073)	167
Managers (58)	9320 (6268)	411	8976 (4527)	112
Professional (75)	8900 (6237)	504	7622 (4394)	152
Father Absent	5610 (5073)	593	5325 (2671)	25
r	.18592		.15736	
b	47.606		39.683	

Figure 15.11

Income by
Father's Occupation

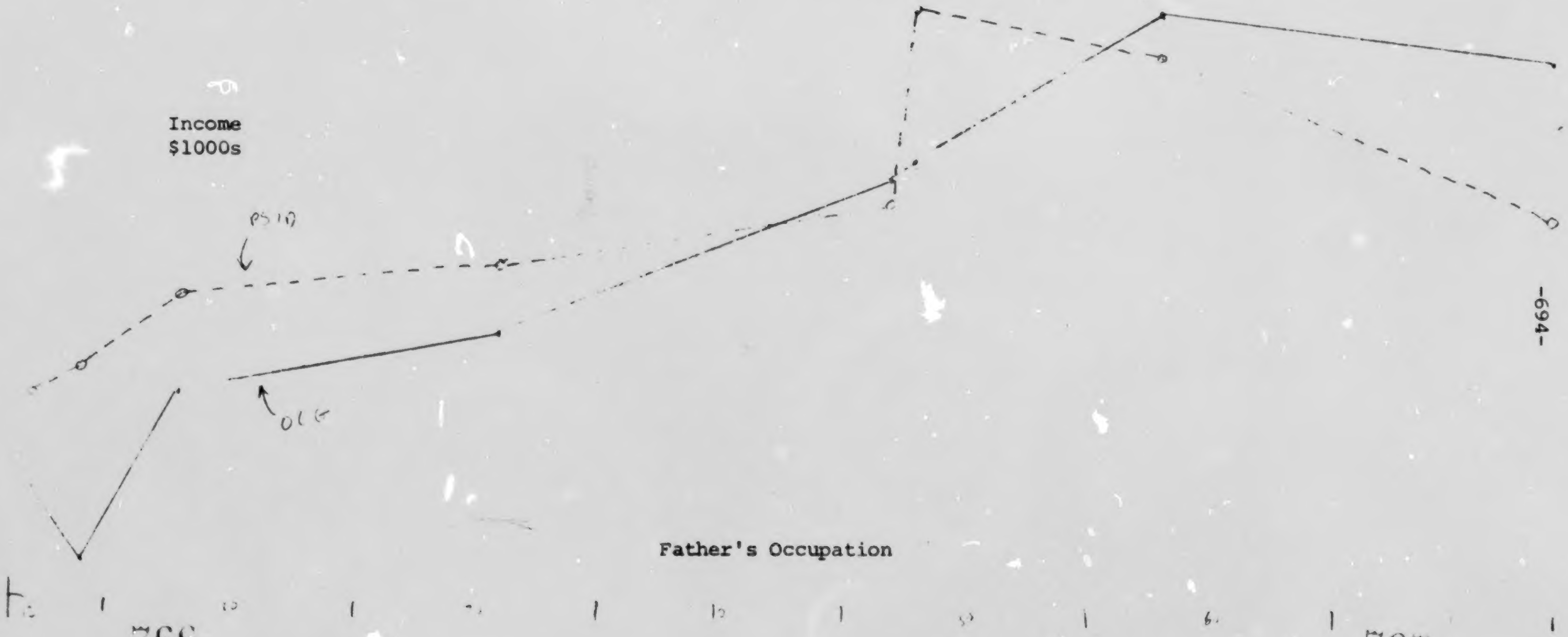


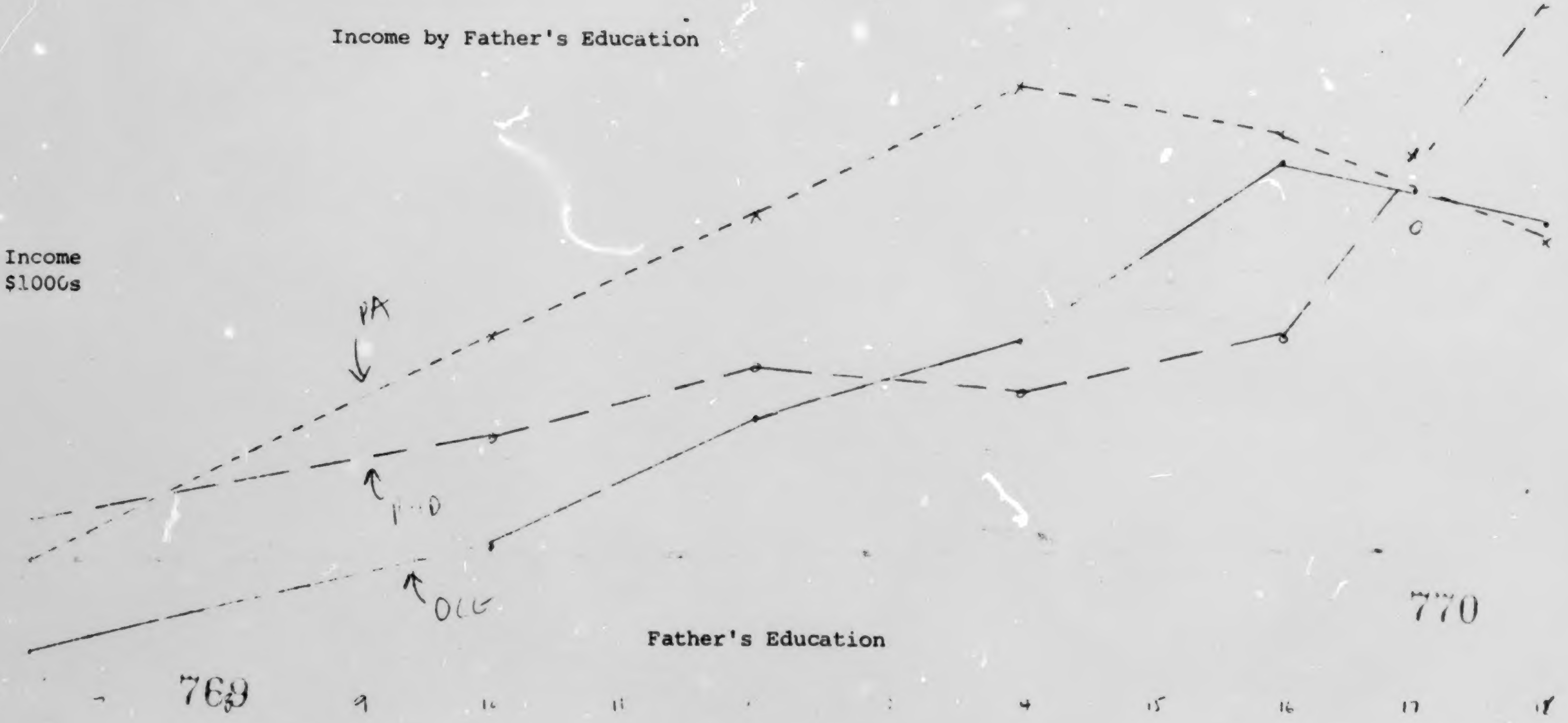
Table 15.12

Income (in 1961 Dollars) by Father's Education for OCG, PA, and PSID Male Household Heads 25-64 with Complete Data and Non-Zero Income

Father's Education	OCG		PA		PSID	
	Mean (S.D.)	N	Mean (S.D.)	N	Mean (S.D.)	N
Total	6251 (4398)	10764	7049 (5082)	1223	7066 (4600)	2300
0-8	5699 (3823)	7130	6403 (4467)	925	6700 (4494)	1570
9-11	6534 (4100)	1024	8164 (5996)	76	7367 (4373)	165
12	7479 (4785)	1363	9030 (5950)	125	7884 (4339)	312
13-15	8070 (5864)	447	10028 (6349)	45	7711 (3792)	101
16	9485 (7315)	292	9732 (7470)	42	8150 (5233)	86
17 or over	9024 (5920)	183	8875 (5508)	10	10757 (8289)	42
Father Absent	5370 (4922)	326	-		5325 (2671)	25
r	.13421		.22551		.14186	
b	199.97		406.98		210.34	

Figure 15.12

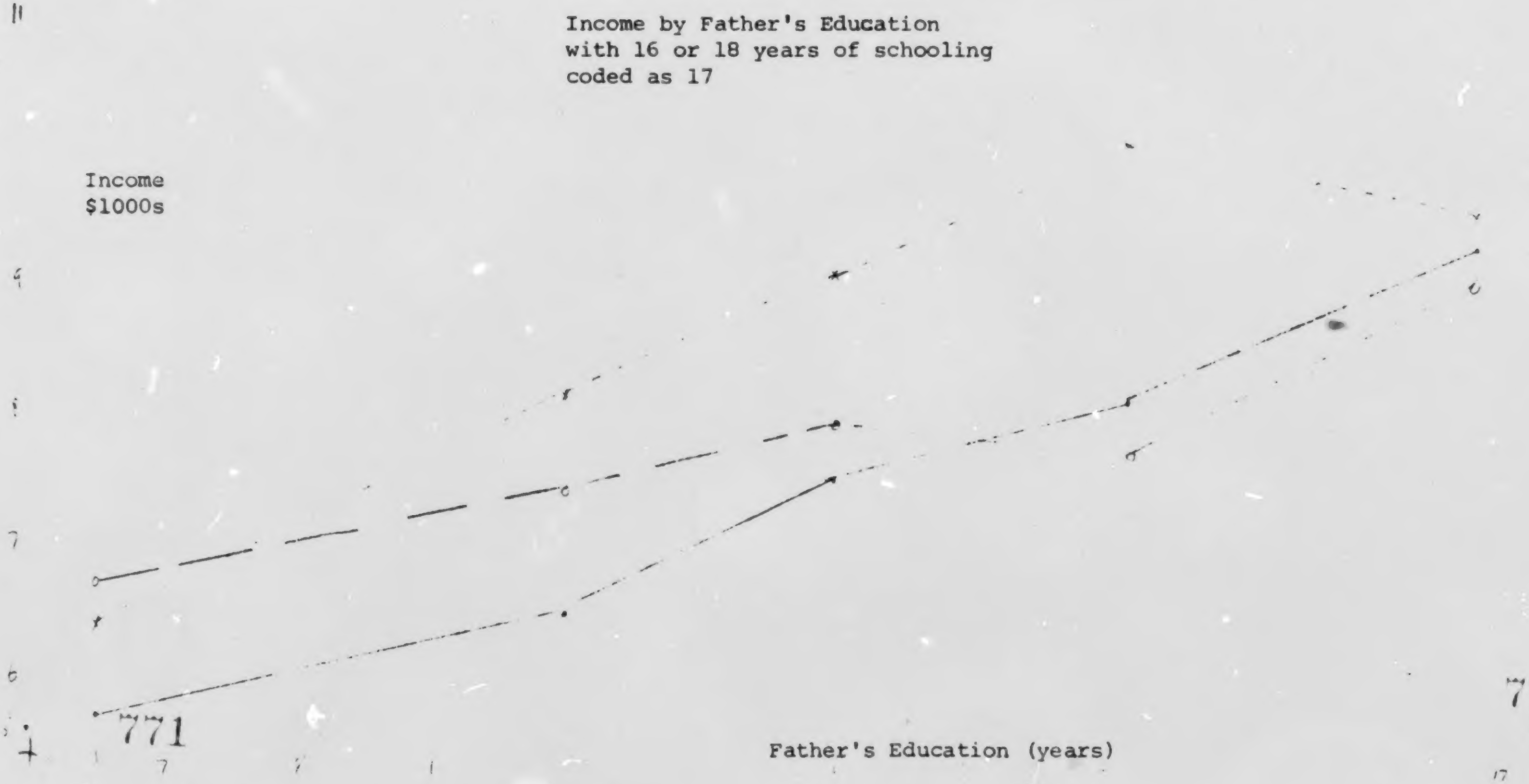
Income by Father's Education



769
770

Figure 15.12 a

Income by Father's Education
with 16 or 18 years of schooling
coded as 17



One possible reason for these differences among samples is that the top category of Father's Education includes all those with 7 or more years of schooling in the OCG but includes only those with a graduate degree in the PA and PSID. One test of this explanation is to calculate a mean Income for the combined 16- and 18-year categories. For the OCG, this procedure yields a mean of about $9300, for the PA, $9,560, and for the PSID, $9000 (see Figure 15.12a). Thus the incomes of men with well-educated fathers in the three samples are not so different after all.

But this test conceals as much as it discloses. If the miscoding explanation is correct, the PA and PSID plots for the 16 and 18 year categories should be similar to each other and different from the OCG; this is not the case. Moreover, even after collapsing the education categories the PA line rises monotonically to the 14-year category and then falls; the OCG and PSID lines rise steadily.

These patterns in the effect of Father's Education are not apparent in the regression coefficients, which suggest that the average impact of a year of Father's Education on income in the PA is about twice what it is in the OCG and the PSID. This is due, as it was for Education and Occupation, to the preponderance of respondents with little-educated fathers in all these samples. It is compounded, for Income, by the tendency of variances in all three surveys, and particularly the PSID, to increase as the mean increases.

The plots of mean Income by Siblings, in figure 15.13, resemble those for Occupation in Table 15.4, which is to say the samples are very similar. The within-category variances are relatively larger for Income than they were for Occupation, so the point estimates for the lines are less reliable. This may help account for the jagged plots.

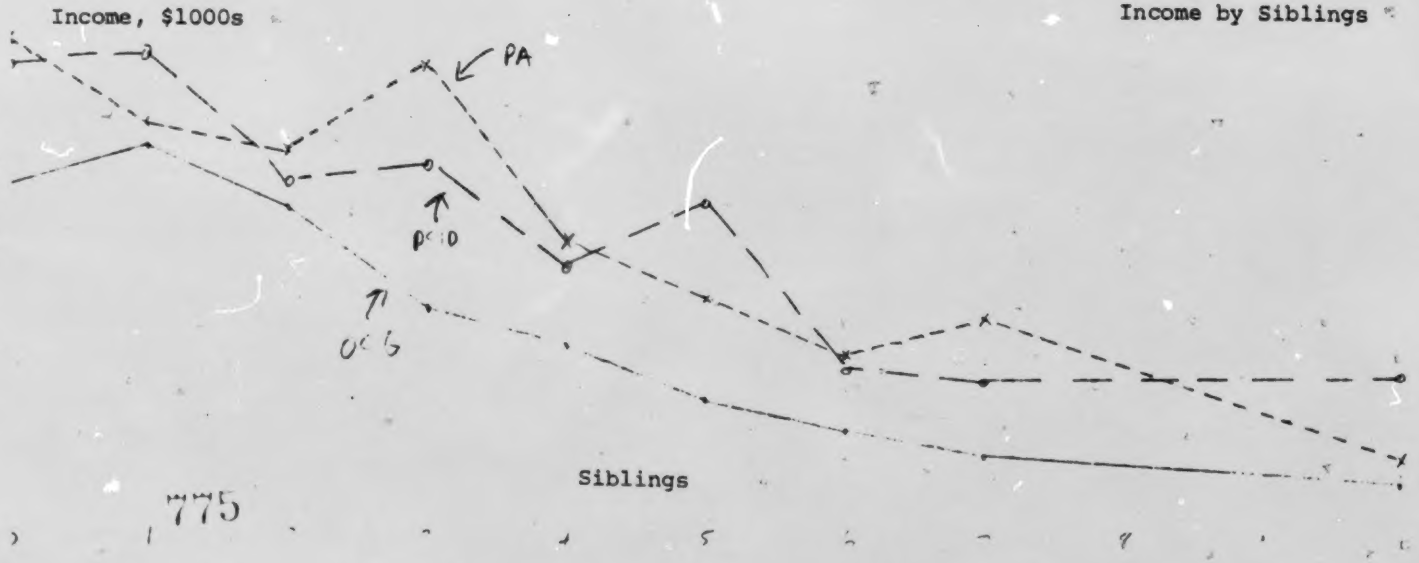
Table 15.13

Income (in 1967 Dollars) by Number of Siblings for OCG, PS, and PSID Male Household Heads 25-64 with Complete Data and Non-Zero Income

Number of Siblings	OCG Mean (S.D.)	OCG N	PA Mean (S.D.)	PA N	PSID Mean (S.D.)	PSID N
Total	6251 (4397)	10770	7049 (5082)	1223	7066 (4601)	2301
0	7266 (5116)	628	8316 (6660)	91	8150 (5279)	143
1	7577 (5262)	1308	7711 (5316)	181	8203 (5532)	373
2	7116 (4740)	1611	7588 (4935)	212	7279 (4037)	363
3	6397 (4349)	1484	8189 (5819)	152	7406 (5068)	352
4	6157 (4344)	1194	6960 (5181)	126	6684 (4105)	281
5	5777 (3833)	1017	6454 (4720)	124	7155 (5029)	197
6	5528 (3704)	844	6032 (4212)	95	5972 (3049)	160
7	5378 (3134)	707	6386 (4870)	68	5865 (3573)	139
8+ (10)	5159 (3741)	1978	5348 (3153)	174	5893 (3694)	293
r	-.11612		-.17884		-.16191	
b	-180.61		-296.82		-252.10	

Figure 15.13

Income by Siblings



The plots of mean Income by Education, in Figure 15.14, are remarkably similar. The impact of years of schooling beyond some college is larger in the PA and PSID than it is in the OCG and Census samples. The regression coefficients reflect this difference, which is probably due to the fact that years beyond the sixteenth always imply advanced degrees in the first two samples.

The association of Occupation with Income, in Figure and Table 15.16, is fairly consistent from sample to sample, but there are some exceptions. The plots are not smooth. Instead, they are quite linear -- and indeed almost coincident -- for blue-collar and clerical/sales occupations (Duncan scores 12, 14, 31, and 47). Moreover, the variances around the line are small and consistent in this range. Self-employed businessmen (48) have unsystematically different mean Incomes in the four samples,

and the corresponding variances are twice what they were for lower-status respondents. This may be due to inconsistencies in the definition of "self-employed" in the different surveys. In the Census, manager/official and professional/technical respondents had about the same mean income as self-employed businessmen. In the other samples, mean Income was highest for manager/officials. These ill-formed relationships combine with different Occupation distributions to yield different regression coefficients in the four samples.

Can Different Samples Be Made to Yield Similar Results?

It is obviously possible to make different samples yield similar distributions and relationships. Indeed, this is what "weighting for nonresponse" tries to do. Thus I need not ask whether survey results can be made similar, but rather how similar they become when certain definitions are applied uniformly.

Table 15.14

Income (in 1961 Dollars) by Education for OCG, PA, Census, and PSID Male Household Heads 25-64 with Complete Data and Non-Zero Income

Education	OCG Mean (S.D.)	OCG N	PA Mean (S.D.)	PA N	Census Mean (S.D.)	Census N	PSID Mean (S.D.)	PSID N
Total	6251 (4397)	10770	7049 (5082)	1223	6432 (4629)	33738	7066 (4601)	2300
0-8	4280 (2695)	2992	4414 (3097)	296	4359 (3147)	7209	4559 (2892)	414
9-11	5538 (3576)	2051	6293 (4246)	233	5391 (3171)	6537	5709 (2571)	374
12	6375 (3430)	3075	7115 (3768)	366	6317 (3660)	10544	6863 (3562)	710
13-15	7582 (5155)	1124	8323 (4923)	141	7326 (4798)	4026	7354 (4376)	380
16	9488 (5837)	877	10435 (6943)	143	9491 (5991)	2676	10017 (5949)	275
17 and over	10310 (6770)	651	13148 (8938)	44	10503 (7235)	2748	12282 (6935)	148
r	.23747		.41346		.38070		.42271	
b	300.91		631.18		493.17		587.22	

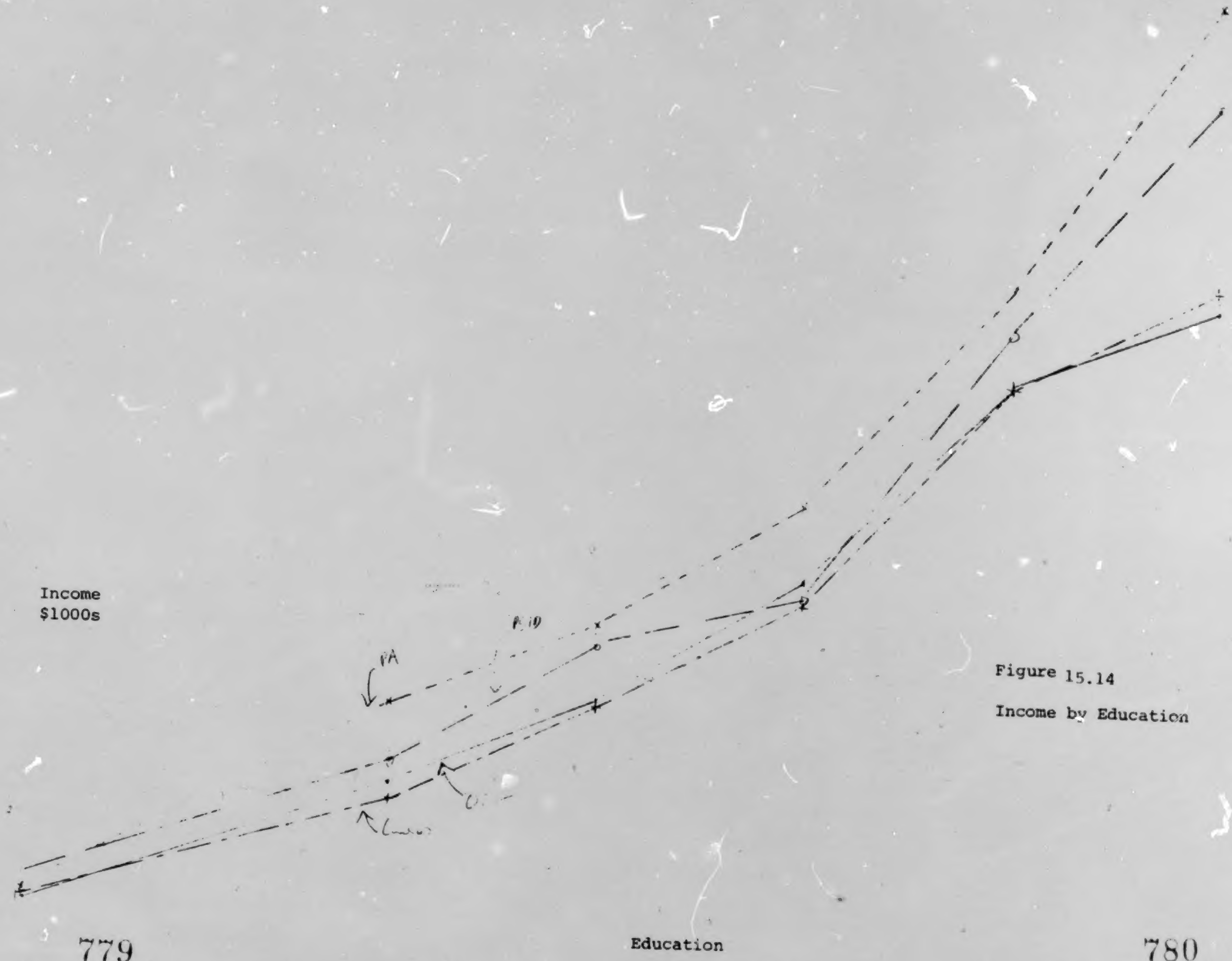


Figure 15.14

Income by Education

Table 15.15

Income (in 1961 Dollars) by Experience for OCG, PA, Census, and PSID Male Household Heads 25-64 with Complete Data and Non-Zero Income

Experience	OCG Mean (S.D.)	OCG N	PA Mean (S.D.)	PA N	Census Mean (S.D.)	Census N	PSID Mean (S.D.)	PSID N
Total	6251 (4397)	10770	7049 (5082)	1223	6435 (4626)	33738	7066 (4601)	2301
0	3532 (3436)	12	-		4039 (2552)	71	5297 (5393)	2
1-5	6088 (3344)	289	6274 (2843)	41	6010 (3496)	1149	5548 (2595)	118
6-10	5278 (3659)	1066	6638 (3456)	127	6007 (3310)	3750	6632 (3886)	301
11-15	6750 (4388)	1430	6850 (3312)	133	6693 (4268)	4086	6943 (3686)	303
16-20	6593 (4255)	1520	7828 (5967)	160	7085 (4769)	4007	8124 (5758)	261
21-25	6787 (5038)	1522	7584 (4825)	166	7223 (5149)	4123	8352 (5088)	312
26-30	6509 (4338)	1366	7966 (5905)	160	6929 (4971)	4495	7523 (3878)	310
31-35	6195 (4783)	1231	7192 (5173)	142	6759 (5235)	4023	7053 (4733)	245
36-40	5699 (4322)	1060	7279 (6006)	121	6120 (4810)	3583	7430 (5781)	197
41-45	5181 (4275)	806	5809 (5321)	102	5241 (4155)	2838	5212 (4073)	160
46-50	4469 (3193)	468	4637 (4422)	71	4165 (3370)	1613	4394 (2967)	90
r	-.07230		-.06168		-.07142		-.04349	
b	-26.807		-25.375		-26.774		-16.373	

Table 15.16

Income (in 1961 Dollars) by Occupation for OCG, PA, Census, and PSId Male Household Heads 25-64 with Complete Data and Non-Zero Income

Occupation	OCG		PA		Census		PSID	
	Mean (S.D.)	N	Mean (S.D.)	N	Mean (S.D.)	N	Mean (S.D.)	N
Total	6251 (4397)	10770	7049 (5082)	1223	6433 (4628)	33738	7066 (4601)	2301
Laborer or Service (12)	3886 (2206)	1332	3291 (2041)	115	4082 (2548)	4166	3931 (2199)	205
Farmer (14)	3197 (3255)	632	5729 (5716)	71	4640 (4633)	994	4793 (4180)	87
Operative (18)	5028 (2148)	2033	5636 (2996)	190	4940 (2593)	6352	5129 (2095)	378
Misc (19/29)	-		5983 (2522)	43	-		-	
Craftsmen (31)	5964 (2620)	2282	6320 (2595)	268	5825 (2927)	7946	6260 (2600)	494
Clerical (47)	6233 (3420)	1243	6835 (3997)	136	6681 (4327)	4626	6481 (3015)	266
Self-employed Business (48)	7374 (6395)	804	9772 (7760)	116	9190 (7746)	1182	8439 (6574)	167
Managers (58)	9774 (6199)	989	10459 (6922)	115	9163 (5967)	3188	9973 (5726)	316
Professional (75)	8898 (5534)	1456	9161 (5575)	169	8853 (5920)	5284	9563 (5462)	389
r	.22901		.35096		.36418		.41855	
b	47.982		87.204		79.057		90.419	

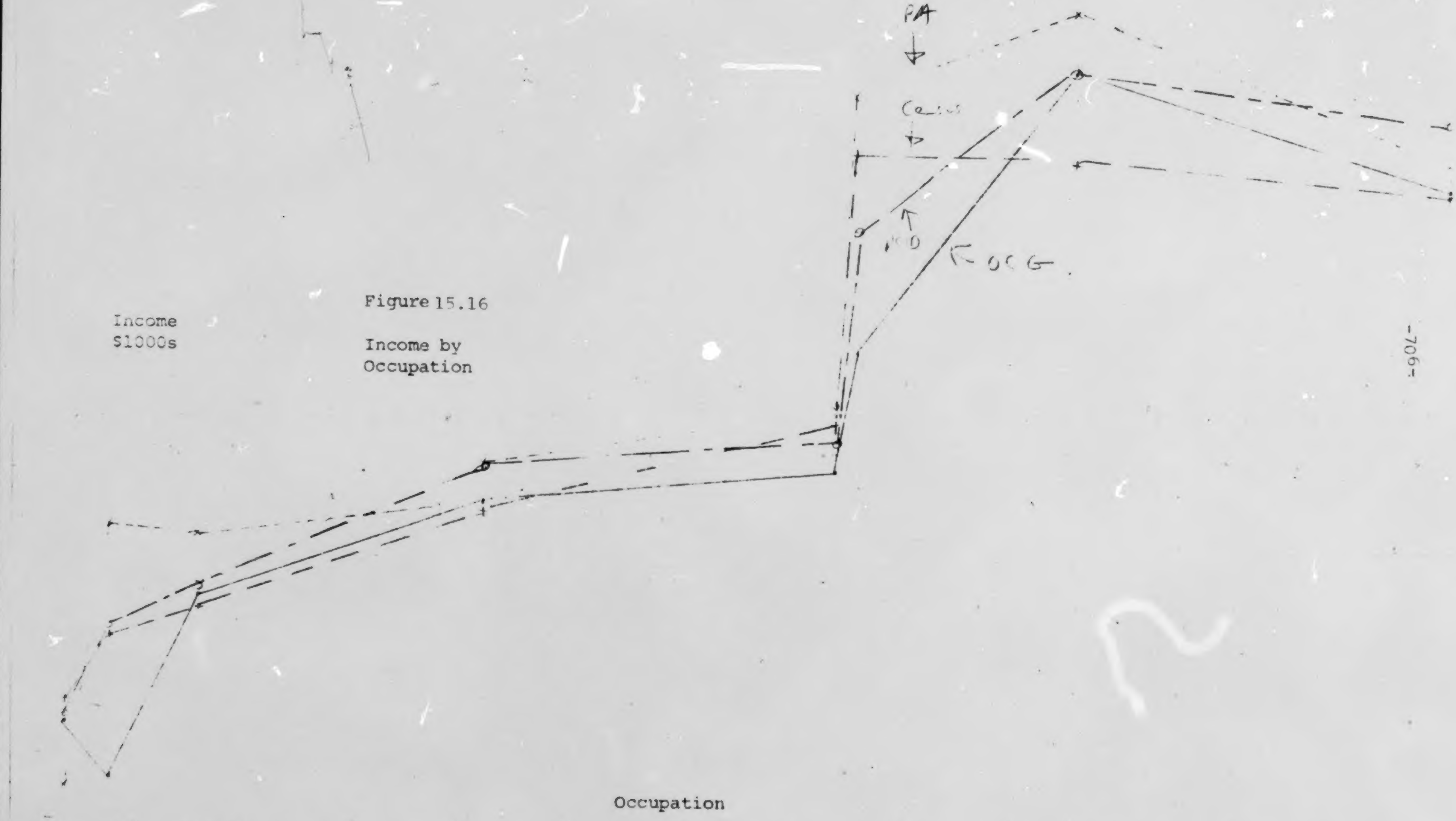


Figure 15.16

Income by Occupation

The answer to my question is simple, at least as far as our model is concerned: survey results become very similar. They do not become identical, nor are the differences random, but on balance the four surveys I have constrained and analyzed provide a consistent view of the relationships between background and economic-success variables. The major differences are as follows:

1. There is a general systematic difference in the surveys over time: the average respondent's education, occupational status, and income rise and the size of his family declines.
2. The Survey Research Center respondents seem to be somewhat better off, in both income and occupational status, than the Census Bureau ones.
3. The average PA respondent whose father got over sixteen years of schooling got less education, entered a lower-status occupation, and earned less than predicted either by simple extrapolation or from the corresponding OCG or PSID data.
4. The average PA respondent with three siblings entered a higher-status occupation and earned more than his OCG or PSID counterparts, although he got no more schooling.
5. The average PA or PSID respondent who got over sixteen years of schooling earned more than his Census or OCG counterparts. This finding is probably due in large part to coding differences for years of schooling beyond the sixteenth.
6. The average income of self-employed businessmen and of managers and officials varies unsystematically from survey to survey. This is not true of other occupational categories.

None of these differences seriously challenges the view that these samples are drawn from roughly the same population. But neither do they permit us to blindly combine multiple-regression results from different surveys. The statistics relevant to least-squares calculations -- in particular, correlation coefficients -- do vary from survey to survey, influenced by small differences in large categories, different clusterings of sample respondents, and the sample differences I listed above.

The influence of large categories and the concentration of a sample's respondents in a particular region of a nonlinear plot are general problems in regression analysis. The nonlinear relationship of, say, Occupation to Siblings in the OCG requires attention even if no other samples are involved. Moreover, such attention is likely to reduce findings' dependence on single coefficients. In doing so, it will reduce the effect of sample-to-sample differences. The analyses we conduct in the rest of this study do in fact pay attention to nonlinearities, and therefore the effect of the differences in the samples' concentrations is diminished.

The analyses in previous chapters made far less effort than I have made to ensure comparability between samples. They are thus able to capitalize on the strengths of each sample. By so doing, they produce results that differ from sample to sample. I have shown here that these differences do not, in general, stem from inadvertently different samples, for when I define variables and samples similarly only a few major differences are apparent in the basic relationships.

Chapter 16

WHY DIFFERENT SURVEYS YIELD DIFFERENT RESULTS: THE CASE OF EDUCATION AND EARNINGS

By Kent McClelland

. 1 Motivation for This Investigation

A question that comes up again and again in this report is how to account for differences in survey results. Even with surveys that cover exactly the same populations at the same point in time some differences are inevitable, if only because of random sampling error. Surveys that cover different populations at different points in time differ for substantive as well as statistical reasons, and it is not always an easy matter to separate the effects of population trends from variability due to sampling error. A third kind of difference is the most disturbing, however. This is the avoidable difference-- one that is entirely due to differences in the methodological procedures followed by the researchers or research organizations who gathered and analyzed the data. Such differences have no substantive interest. Yet we cannot cope with them by using the statistical techniques that help us deal with random sampling errors. My purpose in this chapter is to show how such avoidable methodological differences can affect our survey comparisons.

Any researcher who tries to analyze data from more than one of the major survey organizations soon learns that while there is agreement on the broad outlines of scientific survey procedure, there is practically no agreement about details. Each of the leading survey organizations (e.g. the Bureau of the Census, the Survey Research Center at Ann Arbor, the National Opinion Research Center at Chicago) has developed a set of survey procedures that it uses consistently from one survey to the next, but the procedures are different for each organization. These differences involve sampling frame and technique, choice of informant, question wording, interviewer training and experience, question and interview response rates, weighting of cases, editing and coding of responses, allocation of values to missing data, differences in definitions of variables, rates of keypunch or other processing errors, computer programs used for statistical analysis, and target population or choice of subsample for analysis.

In preparing the appendices, we went to a good deal of trouble to make arbitrary decisions about how to do the analyses uniformly, so that many of the avoidable differences would be eliminated. One important objective of this chapter will be to assess the probable effects of some of those decisions on the results we obtained. In Chapter 15 Jackson went even further than the appendices in eliminating avoidable differences by narrowing his focus to four nationally representative surveys, and by imposing uniformity even when this involved considerable loss of information. I will narrow my focus even more, by concentrating on two major surveys from the same year and restricting my attention to a single relationship -- that between earnings and education.

The focus on a single bivariate relationship allows me to investigate procedural alternatives more thoroughly than I could for a complicated multivariate model. I chose the education-earnings relationship because our preliminary analyses showed rather dramatic differences from survey to survey in the measured strength of the relationship. These differences were too large to attribute to sampling variation. My choice was also motivated by the important policy implications surrounding the controversial issue of whether effects of education on earnings are relatively weak or relatively strong.

This chapter will try to answer two related questions. The first question is how much any given finding can be changed purely by the manipulation of procedural details. What scope is there for pushing and pulling results into different shapes? I will look only at strictly "legal" procedures--ones within the accepted range of good methodological practice --but the fact that these "legal" manipulations can affect results indicates the extent to which an unethical or overzealous researcher could massage his data to produce the result he wanted.

My second question is to what extent the manipulation of procedural details can iron out apparent differences between results from two or more surveys. When the avoidable differences are removed, do different surveys give similar results? Of course, I will only be able to manipulate some of the avoidable differences. Matters of sampling techniques, questionnaire wording, interviewer training, response rates, transcription errors, and the like are totally outside the control of the secondary analyst.

My strategy for the bulk of the analysis in this chapter will be to deal with only two major surveys: the 1970 Census and the 1970 wave of the Panel Study of Income Dynamics (PSID). Since these two surveys cover virtually the same population and were completed the same spring, population trends are not the source of any differences I find.

In Parts 2 to 6 of this chapter I will focus on the second and more narrow of my two principal questions: "Can careful secondary analysis resolve the differences between results from the Census and the PSID?" Part 2 will cover the procedural differences between the two surveys. Part 3 will show the extent to which parallel sample restrictions resolve the differences. Part 4 will deal with differences between the surveys in measurement reliability. Part 5 will explore possible differences in coverage of self-employment income. In Part 6 I will investigate the question of whether panel designs have special sampling problems by comparing results from the PSID to results from the National Longitudinal Study of Mature Men (Parnes Study).

Parts 7 to 10 of this chapter will mainly focus on the more general question of whether common procedural decisions have an important effect on results. Part 7 draws on data from the Census and PSID to explore the effects of coding, alternative income definitions and functional forms. Parts 8 and 9 use data from several years of the PSID to show the effects of extending the income accounting period and using wives as income informants. Part 10 covers a number of commonly used sample restrictions with data from the PSID and Census. Where the effects of a particular procedural manipulation are in the same direction and of about the same magnitude for the two surveys, I will infer that the procedural alternative would have approximately the same effect on other surveys, as well. Where a manipulation has inconsistent effects, I will look for an explanation in terms of other avoidable differences between the surveys. The danger of this kind of analysis is that fluctuations caused by random sample variation or random errors of response and transcription will be interpreted as having some substantive meaning. **Still, to the extent that my analysis is successful, it provides a guidebook to the way certain arbitrary choices can color research findings.**

2 A Comparison of the Two 1970 Surveys: How the Surveys Differ

This section compares a sample of men from the 1970 Census to a 1970 national sample from the Panel Study of Income Dynamics. (See Bartlett and Jencks, Appendix A and Mueser, Appendix D for other results from these two surveys.) Two well-known survey organizations, the United

States Bureau of the Census and the Survey Research Center of the Institute for Social Research at the University of Michigan, Ann Arbor, are responsible for the two surveys.

The samples they have produced differ in a number of important ways. The Census sample is one of several 1/1000 Public Use Samples from the 1970 Census of Population and Housing. The sample we are using is a stratified, systematic subsample of the five percent of the population that received a long form of the Census questionnaire.[1]

The PSID sample, on the other hand, comes from a longitudinal study that began in 1968. The original sample for the PSID was drawn from two sources: a subsample of low income resondents from the 1967 Survey of Economic Opportunity (conducted by the Bureau of the Census), supplemented by a cross-sectional sample of dwelling units in the 48 contiguous states drawn by a multistage, stratified, cluster technique.[2]

Most Census respondents filled out their own questionnaires; the PSID data comes from personal interviews. Possibly because of the difference in interview techniques, the missing data rates are generally lower for the PSID than the Census. The panel design of the PSID means, however, that the overall response rate for the PSID is much lower. Only 76 percent of the original sample were actually interviewed in 1968. By 1970, the third year of interviewing, only 66 percent of the original cases remained in the sample. SRC augmented the sample by interviewing members of sample households (often grown-up children or divorced spouses) who, in the course of the panel study, established "split-off" households of their own. (This procedure helps to maintain the representativeness of the PSID sample.) The estimated Census response rate for white males varies from 95 percent for the 25-29 year olds to over 97 percent for men 50 and older. For black males the estimated response rate is about 90 percent.[3]

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[1] See U.S. Bureau of the Census, *Public Use Samples of Basic Records from the 1970 Census: Description and Technical Documentation*, Washington, D.C., 1972.

[2] For further information, see the Survey Research Center publication *A Panel Study of Income Dynamics: Study Design, Procedures, Available Data; 1968-1972 Interviewing Years*, (Institute for Social Research, the University of Michigan, Ann Arbor, Michigan, 1972) Volume I, pp. 9-22.

Beyond differences in sample design, interview procedures, and response rate, the samples differ in the way the two organizations processed the data. The original Census sample was weighted to conform to the overall Census count for a number of characteristics. Each household head was given a weight for sex, household size, children under 18, race and age. These weights determined the probability of the selection of the household into the 1/1000 sample with which we worked. The actual selection process was stratified with local areas by sex of household head, household size, children under 18, race, and the owner-renter dichotomy. Before selection, if a record of a person or household was missing due to nonresponse or a data processing error, the Census substituted the information from another nearby unit. In effect, this gives a double weight to some individuals and households to compensate for nonresponse.[4]

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[3] See U.S. Bureau of the Census, Census of Population and Housing: 1970, Evaluation and Research Program PHC(E)-4, Estimates of Coverage of Population by Sex, Race and Age: Demographic Analyses, Washington, D.C.: U.S. Government Printing Office, 1973, pp. 4-7 and 28-29. Nonresponse rates in the 5 percent subsample are likely lower than this estimate, since the Census followed up these respondents more closely than others.

[4] U.S. Bureau of the Census, Op. cit., 1972, pp. 191-198

The PSID sample has also been given weights to compensate for nonresponse, as well as for the deliberate oversampling of low-income respondents (to get adequate numbers for analysis), and the addition of split-off households to the sample. The PSID weights are supposed to offset biases due to differential sample attrition in different geographic areas and in groups with varying demographic characteristics.[5]

Because SRC constructed weights only for the interview years 1968 and 1972, I found it necessary to use the 1972 weights (with slight modifications for respondents who got married between 1970 and 1972) and restrict my 1970 sample to respondents who remained in the panel through 1972.

The two survey organizations also used somewhat different allocation procedures for dealing with cases that have missing data. For missing income amounts the Census allocated the response given by the last person previously processed with the same sex, race, and household relationship, and similar age and employment characteristics. The PSID made income allocations on the basis of the individual's response in the previous year of the survey. Where that was unavailable or the respondent had changed jobs, PSID allocated a response from tables based on education, age, marital status, distance to the center of a city of 50,000, race, sex, the population density of the county, and hours worked.[6]

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[5] See SRC, *Op. cit.*, pp. 24-25.

[6] See U.S. Bureau of the Census, 1972, p. 191, and SRC, 1972, p. 311.

I can describe the education allocation procedures in more detail after presenting the wording of education questions.

Table 16.1 compares the wording of questions, and Table 16.2 compares the coding and frequencies of responses for Years of Education, 1969 Earnings and 1969 Personal Income variables from the 1970 Census and the 1970 PSID.

The Census and PSID Education questions illustrate two competing theories about which aspects of education are most important. The Census question focuses on successful completion of academic grades or years. The PSID question also covers a number of other aspects of the educational experience: literacy; type of schooling (academic or vocational); and degrees attained.[7] Since number of years of schooling is an element of both Education variables, I can make the two nearly comparable by using a "lowest common denominator" coding that differentiates completion of six schooling levels. This standardized coding appears in Table 16.2. Use of the six-category code leads to some loss of detail. In this case, the most serious loss of detail is at the upper end of the education spectrum, college graduate and above, where the Census coding of academic years and the PSID coding of higher degrees are fundamentally incompatible. I grouped all men with four years of college or more in the Census sample and a college bachelor's degree or more in the PSID into a single category with a mean years of education estimate of 17.25. (I estimated means of all these categories from the distribution of responses in the original Census sample and then rounded off to the nearest quarter year.)

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[7] The PSID coders were evidently influenced by a practical as well as a theoretical consideration: the desire to save computer storage space by restricting the education code to no more than ten categories--one computer column.

Table 16.1

Question Language and Coding for Education and Income Variables from the 1970 Census 1/1000 Sample and the 1970 Wave of the PSID

YEARS OF EDUCATION	CENSUS	PSID
Question	1. What is the highest grade (or year) of regular school he has ever attended? (Q 21) 2. Did he finish the highest grade (or year) he attended? (Q 22)	1. How many grades of school did you (Head) finish? (M 3, 1968) 2. (If 6 Grades or Less) Do you have any trouble reading? (M 6, 1968) 3. (If 7 Grades or More) Did you have any other schooling? What other schooling did you have? (M 7-8, 1968) 4. (If College) Do you have a college degree? What degree(s) did you receive? (M 9-10, 1968)
Coding	Never attended school . . . . . Elementary through high school (grade or year) . . . . 1 2 3 4 5 6 7 8 9 10 11 12 College (academic year). 1 2 3 4 5 6 or more	0-5 grades and has difficulty reading . . . . 0-5 grades, no difficulty reading . . . . 6-8 grades . . . . 9-11 grades . . . . 12 grades (completed high school) . . . . . 12 grades plus non-academic training . . . College, no degree . . . College, bachelors degree (A.B., B.S., etc.) . . . . College, advanced or professional degrees (M.A., Ph.D., LLB, BD, M.S., etc.) . . . .

Table 16.1 continued

EARNINGS	CENSUS	PSID
Question	Earnings in 1969--Fill parts a, b, and c for everyone who worked any time in 1969 even if he had no income. (If exact amount is not known, give best estimate.) a. How much did this person earn in 1969 in wages, salary, commissions, bonuses, or tips from all jobs? (Before deductions for taxes, bonds, dues, or other items.) b. How much did he earn from his own nonfarm business, professional practice, or partnership? (Net after business expenses. If business lost money, write "Loss" above amount.) c. How much did he earn in 1969 from his own farm? (Net operating expenses. Include earnings as a tenant farmer or sharecropper. If farm lost money, write "Loss" above amount.) (Q 40)	To get an accurate financial picture of people all over the country, we need to know the income of all the families that we interview. 1. (If Farmer) What were your total receipts from farming in 1969, including soil bank payments and commodity credit loans? What were your total operating expenses, not counting living expenses? That left you a net income from farming of ? (H1-H4, 1970) 2. (If R and Family Owned An Unincorporated Business at any Time in 1969) How much was your (Family's) share of the total income from the business in 1969 - that is, the amount you took out plus any profit left in? (H5-H7, 1970) 3. (Everyone) How much did you (Head) receive from wages and salaries in 1969, that is, before anything was deducted for taxes or other things? (H8, 1970) 4. In addition to this, did you have any income from bonuses, overtime, or commissions? How much was that? (H9-H10, 1970) 5. Did you (Head) receive any other income in 1969 from: a) professional practice or trade? How much was it? b) farming or market gardening, roomers or boarders? How much was it? (H11, 1970)

Table 16.1 continued

EARNINGS continued	CENSUS	PSID
Coding	Variable is sum of amounts in a, b and c. Coding is by hundred-dollar intervals up to $49,999. A code of 50 was assigned for $1-99, 150 for $100-199, etc. Sums of $50,000 or over got a code of 70000.	PSID codes a variable named Labor Income, which is different from the Census definition of earnings. (See text.) To reconstruct a comparable earnings variable, I combined variables coded in dollars to $99,999 with variables coded in nine categories. See text for description. I then recoded estimated dollar earnings to conform to the coding to of Census 1969 Earnings.

16.1
Table / continued

1969 INCOME	CENSUS	PSID
Question	Income other than earnings in 1969--Fill parts a, b, and c. (If exact amount is not known, give best estimate.) a. How much did this person receive in 1969 from Social Security or Railroad Retirement? b. How much did he receive in 1969 from public assistance or welfare payments? Include aid for dependent children, old age assistance, general assistance, aid to the blind and totally disabled. Exclude separate payments for hospital or other medical care. c. How much did he receive in 1969 from all other sources? Include interest, dividends, veteran's payments, pensions, and other regular payments. (See instruction sheet.) (Q 41)	1. Did you (Head) receive any other income in 1969 from: c) dividends, interest, rent, trust funds, or royalties? d) ADC, AFDC? e) other welfare? f) Social Security g) other retirement pay, pensions, or annuities? h) unemployment, or workmen's compensation? i) alimony? child support? j) help from relatives? k) anything else? (Specify) (If "Yes" to any Item, Ask "How much was it?") (H11, 1970) 2. Did anyone (else) not living here now help you (Family) out financially - I mean give you money, or help with your expenses during 1969? How much did that amount to last year? (H12-13, 1970) 3. (If Income from Welfare or ADC, AFDC) Did welfare also help you out in any other way - like with your rent or other bills? About how much did that amount to in 1969? (H14-15, 1970)
Coding	This variable is the total of 1969 Earnings and the amounts in parts a, b and c shown here. Coding is exactly the same as that for the 1969 Earnings variable.	As with PSID earnings, this is a constructed variable. I used income totals derived from the questions here plus the earnings questions less spouse's income (categorized). (See text). Like Earnings, estimated income totals have been recoded to conform to Census coding.

Table 16.2

Frequencies of Responses for Education, Earnings and Income Variables from the 1970 Census and PSID. Sample E: Men 25 to 64 Years Old in 1970 with Complete Data on Age, Sex, Household Status, Education and Earnings. Sample G: Heads of Households 25 to 64 Years Old in 1970 with Complete Data on Age, Sex, Household Status, Education and Income.

Original Coding for Education Census		PSID		Standardized Coding for Education	Census	PSID
Coding (years)	Sample E N = 32549	Coding (years)	Sample E Weighted N = 2338 Unweighted N = 2349	Standard Coding (years)	Sample E N = 32549	Sample E Weighted N = 2338 Unweighted N = 2349
0	.7	0 to 5 (illiterate)	2.0			
1	.2					
2	.5			0 to 5 = 3.0	4.9	4.9
3	.9	0 to 5 (literate)	2.9			
4	1.2					
5	1.5					
6	2.8					
7	3.6	6 to 8	16.3	6 to 8 = 7.5	16.9	16.3
8	10.5					
9	6.1					
10	7.3	9 to 11	16.9	9 to 11 = 10.0	18.9	16.9
11	5.6					
		12	20.0			
12	31.2	12 + non-academic	9.9	12 = 12.0	31.2	29.9
13	4.3					
14	5.6	13 to 15	15.3	13 to 15 = 14.0	11.9	15.3
15	2.1					
16	8.0	16 (B.A.)	10.4	16 and over = 17.25	16.1	16.7
17	2.2					
		18 (graduate degree)	6.3			
19	5.9					
	100.0		100.0		100.0	100.0
No Answer	N = 1018	No Answer	N = 21			

Table 16.2 continued:

	1969 Earnings		1969 Income	
	Census Sample E	PSID Sample E	Census Sample G	PSID Sample G
Weighted N	32549	2338	31171	2297
Unweighted N		2349		2313
Net loss of income	.1	.1	.1	.1
Zero Income	4.7	3.0	.8	.1
$1-999	1.5	1.4	1.6	.7
1000-1999	2.2	1.9	2.7	1.7
2000-2999	2.4	2.3	3.0	2.7
3000-3999	3.6	3.3	4.0	3.6
4000-4999	4.8	5.5	4.9	6.0
5000-5999	6.9	7.2	7.0	6.4
6000-6999	8.7	7.1	8.5	7.7
7000-7999	10.5	9.6	10.4	9.8
8000-8999	10.1	8.3	10.1	8.0
9000-9999	8.4	9.3	8.6	9.5
10,000-11,999	13.7	14.3	14.0	15.0
12,000-14,999	10.3	12.8	11.0	12.6
15,000-19,999	6.6	8.3	7.0	9.8
20,000-24,999	2.4	2.6	2.7	2.7
25,000-49,999	2.5	2.9	2.8	3.1
50,000,and over	.6	.3	.7	.4
	100.0	100.0	100.0	100.0
No Answer	N = 5065	N = 22	N = 6786	N = 63

The Census education question has two parts, and the allocation routines for missing data on the two parts were different. If a respondent answered the "highest grade attended" question but failed to indicate whether or not he had finished that grade, the Census allocation routine assumed the grade had been completed. If the respondent failed to answer both questions, the Census allocated the response given in the last processed case with similar characteristics. Conforming to the decision made by Bartlett and Jencks (Appendix A), I treated the second kind of allocation as missing data for the purposes of deletion, but not the first. Because non-respondents to the question of whether the grade had been finished were spread across the whole education range, including these non-respondents has a minimal effect on the Census education and earnings statistics. (They constitute about 1.7 percent of men 25 to 64 in the Census sample.)

Although the PSID did not make allocations of missing education data, the wording of the PSID question was such that some education non-respondents were, in effect, allocated an education response on the basis of a question about whether they could read. (See Table 16.1) The missing data rate for PSID Education (0.9 percent) is correspondingly lower than that for the Census (2.0 percent). (See Table 16.3) I infer from the PSID codebook, although it is ambiguous on this point, that most of the respondents categorized on the basis of literacy ended up in the "0 to 5 grades" category. There is no way to omit these respondents from the analysis. I did eliminate respondents who failed to answer all of the education questions including the literacy question. The PSID question, thus, forces me to classify as respondents some who would be non-respondents to the Census question. Because the group of added PSID respondents falls at the bottom end of the education distribution, it lowers the PSID mean relative to the Census. The effect on the PSID education mean could be on the order of 0.1 years, if the Census nonresponse percentage is a good estimate of the true PSID rate of nonresponse to the "grades completed" question.

Both the PSID and the Census surveys have income questions that are quite comprehensive, but differences in approach complicate my attempt to construct fully comparable earnings variables. I will discuss first the differences in coverage of income components, then the differences in coding and the steps I took to resolve them.

Questions from the two surveys cover the same major components of income, but differ in their coverage of a number of minor details (see Table 16.1). In the category of basic wage and salary income, both questionnaires ask about wages, salaries, commissions and bonuses, but the Census questionnaire mentions tips, while the PSID questionnaire asks about overtime. With regard to other components of earned income, both questionnaires cover net income from the respondent's own farm, business, or professional practice, but the Census questionnaire adds probes about income from partnerships and tenant farming to its questions; the PSID questionnaire probes for income from the practice of a trade and mentions soil-bank payments and commodity-credit loans in its question on farm receipts. The PSID also adds a question about income from farming or market gardening or from roomers or boarders.

Under the heading of unearned income, both surveys inquire about Social Security payments, welfare, interest, dividends and pensions. The Census question also includes Railroad Retirement, public assistance and, in the question covering "all other sources", asks about "other regular payments." The PSID questionnaire specifies ADC and AFDC payments in its question on welfare income; it probes for income from rent, trust funds and royalties; it also explicitly asks about private transfers in questions about "help from relatives" and "help from non-relatives." The PSID income questions ask for a greater number of separate totals than the Census. The PSID questions are, on the whole, the more detailed of the two sets.

In coding of income responses the differences between the Census and PSID surveys are substantial. The PSID researchers have not coded an earnings figure. Instead, they code a variable called Labor Income. This differs from the Census conception of Earnings in two ways. First, part of an individual's income from his own farm or business is treated as a return on assets and excluded from Labor Income. Second, any net losses the individual may incur from the farm or business are attributed entirely to return on assets; PSID Labor Income is, consequently, never allowed to take on a negative value. To estimate the proportion of farm or business income that should be counted as a compensation for labor, the PSID coders assigned one dollar for every hour worked to Labor Income, then split the remaining self-employment income equally between Asset Income and Labor Income. If the respondent netted from his self-employment a positive amount that was less than a dollar for every hour worked, the whole total was assigned to Labor Income. The estimation procedure I used reversed these steps to find the total amount of farm or business income.

The PSID Labor Income conception has some distinct theoretical advantages, especially if one's object is to get an estimate of returns to human capital that is uncontaminated by returns to other assets. My purpose, however, was to achieve comparability, so I prefered to use the

Census definition of earnings, as has been done elsewhere in this report. I first adopted the algorithm for transforming labor income to earnings used by Mueser (Appendix D), but discovered that it overestimated the earnings of men with self-employment losses and men with both farm or nonfarm business and wage income. These men constitute about 4 percent of the population. My final estimation procedure for deriving an estimate of earnings from the available PSID variables was as follows: a) for men with no asset income from a farm or business, Earnings = Labor Income; b) for men with positive asset income from a farm or business and no income from wages, salaries, overtime, bonuses, commissions, professional practices or trades, Earnings = (2 x Labor Income) - (Hours Worked Last Year); c) for men with farm or business losses and for men with farm or business income, and also income from wages, salaries, overtime, bonuses, commissions, professional practices or trades, Earnings = (Head and Wife's Total Taxable Income) - (Wife's Labor and Asset Income) - (Head's Income from Rent, Dividends, Interest, Trust Funds, or Royalties). Unfortunately, the PSID does not code Wife's Asset Income and Head's Income from Rent, etc. in exact amounts, so I had to estimate them from broadly categorized variables. (The PSID codes all other components of Labor Income in actual dollars, up to $99,999.)

The PSID tape does not provide a dollar figure for Head's Total Personal Income, since the PSID investigators were primarily concerned with family finances. To estimate 1969 Personal Income I used the following equation: Personal Income = (Head and Wife's Total Taxable Income) + (Head and Wife's Transfer Income) - (Wife's Labor and Asset Income + Wife's Transfer Income). It was necessary to estimate Wife's Asset Income and Wife's Transfer Income from broad categories, but this introduces inaccuracies in only a fairly small percentage of cases. (Other components of income were coded in actual dollars up to $99,999.)

For the Census income variables the obstacle to comparability was not a matter of income components and definitions, but a matter of coding. With the stated purpose of protecting the anonymity of respondents, the Census Bureau recorded income variables in a relatively fine-grained categorization rather than in dollar amounts.

The Census code has a category for every hundred-dollar interval under $50,000 (and, for self-employment income, intervals for negative responses down to $9,899). The remaining three categories group responses of $9,900 or less, responses of exactly zero, and responses of $50,000 or more.

Bartlett and Jencks (Appendix A) used category midpoints (50 for 1 to 99, 150 for 100 to 199, etc.) to estimate the means of the categories for the Census income components. They assigned an estimated mean of $70,000 to income amounts of $50,000 and over. To calculate Earnings and Personal Income totals they added up the income components, retaining the sum even when it came to over $50,000. In order to insure comparability, I used this same procedure for both the Census and PSID data, except that I recoded sums over $50,000 to exactly $70,000.

About 12 percent of Census household heads had no earnings data compared to less than 1 percent of the PSID sample. Two factors may be contributing to the difference. The PSID use of interviewers rather than self-reporting forms probably encouraged more complete answers; moreover, individuals of the type who did not want to answer income questions had probably already dropped out of the PSID by 1970.

Elimination of respondents with missing data makes the samples less representative of the target population but controls for the idiosyncracies of the two survey organizations' different allocation routines.

3 Reconciliation of Census and PSID Differences: Basic Sample Restrictions

Table 16.3 shows education and earnings results for samples from the PSID and Census to which some basic sample restrictions have been applied. It also shows the intermediate steps, as restrictions are added one by one. All the samples in Table 16.3 (and subsequent tables) are restricted to men between the ages of 25 and 64 in 1970 with complete data for sex, age, and household relationship. The age limitations duplicate those placed on most of the other sample analyses reported in this book.

The first step reported in Table 16.3 (Sample A compared to Sample B) was to eliminate men who were not heads of households from the Census. I constructed Samples C and D by dropping from Sample B first respondents with missing education data, then respondents with missing earnings data.[8] Sample E has no cases with missing data on either education or earnings. Sample F duplicates sample E, but also omits men with zero or negative earnings. Samples E and F are the ones I use most often in subsequent tables.

We can use Table 16.3 to make two kinds of comparisons. We can look at the statistics from the same survey but differently restricted samples, to see, for instance, what effect restricting the Census sample to heads of households has on the Census means and standard deviations. On the other hand, we can compare the two surveys to see how they differ when we put similar restrictions on the samples. I will begin by assessing

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[8] For samples B and D of the PSID survey, I allocated a value that approximates the sample mean to cases with missing education data. There were 22 such cases. In the Census samples I used the values already allocated by the Census Bureau to non-respondents. For a description of the relevant allocation procedures see Bartlett and Jencks, Appendix A.

Table 16.3

Comparison of Census and PSID Sample Means, Standard Deviations, Correlations and Regression Statistics for Years of Education and 1969 Earnings.

Sample A: All Male Respondents 25-64 Years of Age in 1970
Sample B: Male Heads of Households, 25-64 in 1970.
Sample C: Male Heads of Households, 25-64 in 1970, with complete Education data.
Sample D: Male Heads of Households, 25-64 in 1970, with complete Earnings data.
Sample E: Male Heads of Households, 25-64 in 1970, with complete data on Education and Earnings.
Sample F: Male Heads of Households, 25-64 in 1970, with complete data on Education and Earnings, and Earnings > 0.

	Sample A	Sample B		Sample C		Sample D		Sample E		Sample F	
	Census	Census	PSID	Census	PSID	Census	PSID	Census	PSID	Census	PSID
Weighted N	41251	37237	2381	36504	2359	32763	2359	32549	2338	30969	2268
Unweighted N			2393		2366		2374		2349		2255
Education Mean	11.3747	11.4713	11.6691	11.4932	11.6693	11.4940	11.6672	11.5017	11.6674	11.6248	11.7583
Education S.D.	3.6214	3.5762	3.6014	3.5752	3.6177	3.5831	3.6020	3.5823	3.6182	3.5176	3.5790
S.E. of Educ. Mean	.01783	.01853	.07364	.01871	.07439	.01980	.07394	.01986	.07470	.01999	.07538
Earnings Mean	8837.68	9323.25	9899.56	9360.63	9909.23	9311.66	9829.76	9329.99	9838.39	9809.59	10146.88
Earnings S.D.	7480.73	7584.02	6891.46	7618.29	6896.41	7476.00	6804.91	7483.27	6809.23	7354.98	6678.03
S.E. of Earnings Mean	36.83	39.30	140.91	39.87	141.81	41.30	139.69	41.48	140.52	41.80	140.66
Correlation Coefficient	.36042	.35870	.43401	.35972	.43565	.37026	.43920	.37020	.44089	.35639	.42990
Unstandardized Regression Coef.*	744.52	760.70	830.49	766.53	830.48	772.54	829.73	773.32	829.72	745.17	802.13
S.E. of Regression Coefficient	9.49	10.26	35.25	10.41	35.29	10.71	34.84	10.76	34.86	11.10	35.49
R^2	.12990	.12867	.18837	.12940	.18979	.13709	.19290	.13705	.19438	.12701	.18481
S.D. of Residuals	6978.04	7079.41	6209.86	7108.40	6208.90	6944.78	6114.76	6951.71	6113.01	6872.15	6030.78

* Earnings the Dependent Variable

the effects of the sample restrictions on each survey separately. Then I will proceed to a comparison of the two surveys.

9.7 percent of the men between 25 and 64 in the original Census sample were not heads of households.[9]

Men who are not heads of households differ from other men between 25 and 64 in a number of ways. In the Census sample about half of the men who are not heads of households are less than 35 years old. They are about twice as likely as household heads to be non-white, educated eight years or less, or still attending school. About 15 percent of the non-heads have a disability which prevents them from holding a job. They therefore make up over half of those with zero income. Non-heads are more likely than heads of households to have had less than fifty weeks work in 1969, to have occupational status scores under thirty, and to have total personal incomes less than $6,000. Removing household non-heads from the Census (Sample B compared to Sample A) increases the mean for Years of Education by 11.47 - 11.37 = 0.10 years and the mean for 1969 Earnings by $9323 - $8838 = $485. (Since the PSID tape I used contained only heads of households, Sample A is not available for the PSID.) The Census heads-of-households sample also has slightly larger standard deviations for education and earnings. Changes in the correlation coefficient and unstandardized regression coefficient are negligible.

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[9] In a comparison of the Census to the PSID individuals tape Mueser found about twice as many household non-heads in the Census as in the PSID. (See Appendix D.) It seems reasonable to me that the Census count would be more accurate, since the Census is in the business of finding every individual, while the PSID sample is oriented much more strongly toward households. Nonetheless, it must be noted that evidence from the PSID

The deletion of cases with allocated data on education and earnings (compare Samples E and B) produces only one noteworthy change in the results for either survey: an increase of about 0.01 in the Census earnings-education correlation coefficient. Since the Census does not make its allocations on the basis of the education-income relationship, it is reasonable that eliminating allocations should increase the correlation. The only surprising thing about the result is that, with over 12 percent of the Census heads of households having allocated values, deleting them makes so little difference.

The difference between Samples E and F in Table 16.3 is the deletion of respondents with zero or negative earnings, a sample restriction used in most of the other chapters of this book. This group of extreme cases constitutes nearly five percent of the Census sample but only about three percent of the PSID sample. Excluding them raises the Census mean by $480 and the PSID mean by $309. The earnings standard deviation is reduced by about $130 for both surveys. Low education is associated with zero earnings, so eliminating these cases raises the education mean by about 0.1 years in both surveys. Negative earners tend to come from the middle of the education distribution. Eliminating men with zero or negative earnings lowers the correlation coefficients for both surveys by just over 0.01 and lowers the unstandardized regression coefficients by $28. To sum up, the exclusion of zero and negative earners has a substantial impact on earnings means. It has a very small effect on the earnings-education regression statistics.

How do the basic sample restrictions affect the comparison between Census and PSID results? In samples restricted to heads of households (Sample B) the PSID education mean is greater than the Census mean by

11.67 - 11.47 = 0.20 years. Removal of education and earnings allocations reduces the difference to 0.17 years (Sample E). Eliminating zero or negative earners reduces the apparent difference to 0.13 years of education (Sample F). I must stress that these are only "apparent" differences, because in the treatment of missing education data the PSID mean is biased downwards relative to the Census (see section 1 above). If as many as one percent of the PSID respondents failed to report education, were assigned to the "zero to five grades" category on the basis of their answers to the literacy question, and were actually like the men who failed to report to the Census, this would bias the PSID mean by 0.09 years relative to the Census. The true Census-PSID difference, then, may be as much as 0.29 years for Sample B, 0.26 for Sample E and 0.22 for Sample F.

Whether the differences between the education means from the Census and PSID are statistically significant is a matter of conjecture. Because the PSID is by far the smaller of the two surveys, standard errors of the differences in survey means depend mostly on the size of the PSID standard errors. The cluster design of the PSID sample makes standard errors for the PSID larger than for a random sample of the same size, and weighting affects standard errors in unpredictable ways. Morgan et al. conclude that true standard errors for multiple regression coefficients based on the PSID data may be larger by a factor between 1.2 and 1.8 (depending on the variables involved) than standard errors calculated by the ordinary formulas. They report that the design effect for standard errors of bivariate regressions is likely to be not quite as large.10/

10/ James Morgan (ed.), 1974, Five Thousand American Families--Patterns of Economic Progress, Vol. 1, Institute for Social Research, University of Michigan, Appendix B.

If I take 1.5 as an arbitrary estimate of the amount by which my calculated PSID standard errors must be increased, the standard error for the difference of the education means in Sample E could be $((0.07470 \times 1.5)^2 + 0.01986^2)^{0.5} = 0.1138$. Consequently, a difference of $0.1138 \times 1.96 = 0.22$ between the Census and PSID means could be statistically significant at the 0.05 level. I conclude that, taking into account the difference in treatment of missing data, the Census-PSID differences are probably significant for Samples B and E and may be marginally so for Sample F. Though this conclusion must be taken with a great deal of caution, it points to a systematic PSID oversampling of highly educated respondents (or undersampling of poorly educated respondents) as an important difference between the Census and PSID. Education standard deviations from the two surveys show only negligible differences.

PSID samples also have higher mean earnings than similarly defined Census samples. The heads of households samples (Sample B) differ by $9899 - $9323 = $576. Removal of allocated earnings and education data (Sample E) lowers the difference to $508. The samples with positive earnings (Sample F) differ by only $337. Assuming once again that PSID standard errors must be inflated by a factor of 1.5, the standard error of the difference in earnings means for Sample E is $((140.52 \times 1.5)^2 + 41.48^2)^{0.5}$ = $214.82, and a significant difference is $215 x 1.96 = $421. This suggests that the Census and PSID earnings means are probably significantly different in samples that include zero earners, but once respondents with zero or negative earnings are omitted, the remaining differences are nonsignificant. Part of the remaining differences can be explained by the difference between the surveys in Years of Education. In Sample F, for instance, the Census and PSID education means differ by an estimated 0.22 years (allowing for differences in treatment of missing data). On the basis of the Census Sample F regression coefficient one would expect surveys with that much educational difference to differ in earnings by 745 x 0.22 = \$164; in fact, they differ by \$337.[11] Jackson also reports that 54.7 percent of Census household heads held blue-collar jobs compared to 46.8 percent of PSID respondents. This would help explain differences in earnings.

While earnings means from the PSID and Census show some tendency to converge as I add sample restrictions, substantial differences in the variance of the earnings distributions persist. Even for Sample F the standard deviations of Census and PSID 1969 Earnings differ by \$7355 - \$6678 = \$677. The F-test for the significance of the difference yields a ratio of $7355^2/6678^2 = 1.21$, which is significant at the 0.01 level. Census earnings distributions have relatively greater numbers of extreme cases than comparable PSID distributions (see Table 16.2).

When one regresses earnings on education, R^2 for the PSID samples is consistently about 0.06 higher than for the Census. Sample restrictions appear to have little effect on this difference. The Census-PSID divergence in earnings variance and R^2 may be due either to differences in measurement reliability between the surveys, or to sampling differences, or to some combination of these two factors.

In the next section I will explore the possibility that differences in measurement reliability account for the differences in Census and PSID correlations.

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[11] The presence of PSID respondents whose education has been "allocated" by literacy probably biases the PSID Earnings mean as well as the Education mean with respect to the Census. My guess is that they lower the Earnings mean by an amount less than fifty dollars. For evidence on this point, see the discussion of Table 16.7 below. If my estimate is correct, the observed PSID-Census earnings differences slightly understate the true differences. but none of the conclusions about significance needs to be

4. Reconciliation of Census and PSID Differences: Measurement Reliability

There is reason to believe that PSID earnings are more reliably measured than Census earnings. Since PSID interviewers returned annually to the same respondents, the respondents knew ahead of time what questions to expect. The PSID researchers cite instances of respondents having tax records ready to help in making accurate replies to income questions. It seems likely that PSID respondents took more time and care in their answers than the average Census respondent. Because we have measurements of PSID earnings from several years, we can calculate a lower-bound estimate of PSID earnings reliability on the basis of the simple Markov model that **assumes** true earnings in year one are not negatively correlated with true earnings in year three even with earnings in year two controlled (see Jencks, Chapter 13). Applying this model to the subset of my Sample F that has no allocated data for any of the five years from 1968 to 1972, I calculated the reliability of 1970 PSID earnings as at least $(r_{69.70} \times r_{70.71}) / r_{69.71} = (0.86998 \times 0.87138) / 0.86140 = 0.88$ where r stands for the correlation between earnings measurements in the subscripted years.

I am not aware of any published estimates of the reliability of 1970 Census Earnings, but the Bureau of the Census has published the results of a study that matches the responses of individuals who participated in both the 1970 Census survey and the 1970 Current Population Survey (CPS). I calculated correlation coefficients from the cross-tabulations of Census and CPS reports of income for the same individuals. If Census and CPS reports are equally accurate, these correlations can be taken as reliability coefficients. The correlation for the wage or salary income of 5036 men, aged 14 and over, who report non-zero wages in both surveys is 0.84. The correlation for total income of the 6443 men over 14 who reported non-zero income in both surveys was 0.78. The correlation for the 343 men who reported nonfarm self-employment earnings in both surveys was 0.69.[12/] These correlations are biased downward because they are calculated from categorized income data. Applying the same categorization to PSID earnings lowers the interannual intercorrelations by an average of 0.04. I estimate, then, that the true correlation between the CPS and Census wage reports is about 0.84 + 0.04 = 0.88.

The assumption that Census and CPS earnings reports are equally accurate may be false. The standard deviation of the Census wage reports for men in the matched sample is $6257., while the standard deviation of the CPS reports is $5820. If the difference in standard deviations were due different levels of random error, we could estimate the Census reliability as (0.88)(5820/6257) = 0.82, and the CPS wage reliability as (0.88)(6257/5820) = 0.95. This estimate for CPS wage reliability is perhaps higher than one would expect, but CPS uses experienced interviewers, rather

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12/ The original tabulations are in U.S. Bureau of the Census, Accuracy of Data for Selected Population Characteristics as Measured by the 1970 CPS-Census Match, PHC(E)-11, Washington, D.C., 1975. Tables 48, 49 and 50, pp. 97-99.

than self-report forms, to gather data. Evidence from another Census Bureau matching study indicates that, given certain assumptions, the high estimate for CPS reliability is not unreasonable.

In 1973 the Census matched 1972 income reports from the CPS to income tax returns and Social Security income reports for the same people. 33,390 husband-wife couples reported non-zero 1972 wage or salary income to both the CPS and the IRS. Their reports correlated 0.90.[13/] The estimate, again, is from categorized data, so the true CPS-IRS correlation is likely to be about 0.94. The mean of the CPS reports is about 98.5 percent of the IRS mean, and the standard deviations of the two wage and salary distributions are approximately equal ($7285. for CPS and $7330. for IRS).

The interpretation of these data depends on our assessment of the accuracy of the IRS reports. If we assume that tax returns are completely reliable, the correlation of the CPS report with the IRS criterion is equal to the square root of the CPS reliability. Under this assumption, the reliability for CPS wage and salary income reports would be $0.94^2 = 0.88$. But, given the assumption of completely accurate IRS reports, the equality of IRS and CPS variances we observe could only be due to errors in the CPS reports that are negatively correlated with true wages. (It might happen, say, that persons with high salaries tend to under-report to the CPS, while respondents in the lower wage brackets make essentially random errors.) However, I found that the correlation between matched Census and CPS wage

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[13/] I have calculated the correlation coefficient from Table 5 in Roger A. Herriot and Emmett F. Spiers, "Measuring the Impact on Income Statistics of Reporting Differences Between the Current Population Survey and Administrative Sources,"pp. 29-39 in U.S. Bureau of the Census, Some Preliminary Results from the 1973 CPS-IRS-SSA Exact Match Study, Washington, D.C., 1975. (These papers were delivered at the 1975 annual meeting of the American Statistical Society and appear in the 1975 Proceedings of the Social Statistics Section.) My calculation is on the basis of estimated means assigned to the cells of a 16 category by 16 category cross-tabulation of income reports.

reports was also about 0.88, so this line of argument would force us to conclude that the reliabilities of the CPS and Census wage reports were approximately equal, although the variances of their distributions are strikingly different. Tendencies toward negatively correlated errors in the CPS would have to be absent from the Census. The alternative assumption is that CPS and IRS reports are equally faulty indicators of true wages. In this case we could assume that all errors were random, and that the reliabilities of CPS and IRS wage reports would both equal 0.94. This reliability figure corresponds closely to the independently derived estimate of CPS wage reliability on the basis of the Census-CPS match and similar assumptions about random errors. My inclination is to accept the second assumption of IRS fallibility and random errors and to conclude that CPS wage reliability is about 0.94 or 0.95 and Census wage reliability is about 0.82.[14/]

What I am really interested in is the reliability of earnings, not wage and salary reports. Earnings include self-employment income, which is less reliably measured than wages. But self-employment income makes a small enough fraction of total earnings that the overall reliability should be decreased only slightly. I would guess that earnings reliability would be about 0.01 or 0.02 less than wages reliability. My interest, too, is confined to estimating reliabilities for respondents between the ages of 25 and 64, not 14 and over. My experiments with the PSID sample, however, indicate that the width of the age range has little impact on the reliability estimate. I conclude, then, that the reliability of Census earnings for men 25 to 64 is about 0.81.

[14/] For a discussion of errors in IRS reports, see Simon Kuznets, 1953, Shares of Upper Income Groups in Income and Savings, New York: National Bureau of Economic Research, Chapter 11.

As long as measurement error is random with respect to the variable being measured, the measured variance will be equal to the sum of the true population variance and the variance attributable to measurement error. The reliability coefficient is then equal to the ratio of the true population variance to the observed variance. If errors were random, then, the true variance for the Census Sample F would be $(0.81)(7355)^2 = (6619)^2$ while the true variance for PSID Sample F would be $(0.88)(6678)^2 = (6265)^2$. These values still differ significantly, so we cannot explain the difference in observed variances solely in terms of the greater reliability of the PSID responses.

The bivariate regression results depend on the reliability of both the education and the earnings measurements. In the 1970 CPS-Census matching study, education reports for men over 25 correlated 0.87; if Census reports were as reliable as CPS reports, this correlation would estimate the reliability of the Census education measure.[15] The standard deviations of the two sets of reports are virtually identical (Census 3.52; CPS 3.53), so I will proceed on the assumption of equal reliability.

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[15] Data from U.S. Bureau of the Census, 1975, *Accuracy of Data for Selected Population Characteristics as Measured by the 1970 CPS-Census Match*, PHC(E)-11, Washington, D.C.: U.S. Government Printing Office, Table 24, p. 44. I applied my standard 6-category code for education to the subtable for men 25 years old and over. Since men over 65 are included in this correlation, and reliability apparently deteriorates with age, the reliability for a 25 to 64 year old sample would be slightly higher than 0.87. The pattern of marginals suggests that CPS responses are more likely to be rounded off than Census responses, so the Census responses may be more accurate. For more on education reliability, see Olneck, Chapter xx, and Bishop.

In the 1975 wave of the PSID, SRC asked respondents about their education level a second time, making possible an estimate of PSID education reliability from the correlation of the two reports. The two reports (coded in my six-category code) correlate about 0.90 for the male respondents between 25 and 64 in 1970 who were heads of households from 1968 to 1975. This is probably a lower bound estimate of reliability because the accuracy of education reports declines for older respondents and some younger respondents get additional education. The correlation for a sample of respondents at least 35 in 1968 and no older than 64 in 1975 is only 0.91, however, so these factors are relatively unimportant.

With estimates of earnings and education reliability available for both surveys, I am able to apply the familiar correction for attenuation (McNemar, 1969) to the earnings-education correlation coefficients from the two surveys, to see whether reliability differences account for the differences between correlations, and, by extension, the differences between variances explained (R^2 coefficients). Using Sample F, again, I find an estimated true correlation coefficient for the PSID to be $0.42990 / (0.88 \times 0.90)^{0.5} = 0.48$ and an estimated true correlation coefficient for the Census to be $0.35639 / (0.81 \times 0.87)^{0.5} = 0.42$. Thus, a difference in correlations of 0.06 remains even after correcting for attenuation. Correcting for unreliability leaves the difference in R^2 unchanged. I conclude that different levels of measurement error do not provide an explanation of observed differences in the strength of the education-earnings relationship between the PSID and Census.

Unstandardized regression coefficients are only affected by measurement error in the independent variable -- education, in this case. My estimated reliabilities for Census and PSID education are not different enough to account for any appreciable amount of the difference in regression coefficients.

5 Coverage of Self-Employment Earnings

In this section I pursue the possibility that the reason for the differences in PSID and Census results may be differences in their coverage of various components of earnings. It is generally thought that wage and salary income, which is by far the largest component of earnings, is more fully and reliably reported in surveys than the components of earnings deriving from various kinds of self-employment income.[16]
In the CPS-IRS matching study cited in Part 4, the mean of the CPS reports was better than 98 percent of the mean wage and salary income reported to the IRS.[17]

I found that Census reports of wage and salary income had a mean about 2 percent higher than CPS reports for respondents in the Census-CPS match. If wage and salary income is fully covered in most surveys, it seems reasonable to expect that non-reporting or under-reporting of self-employment income will be the main source of discrepancies in survey earnings totals.

Table 16.4 divides the Census and PSID Sample E into four mutually exclusive subsamples. Subsample E.1 includes men who report 1969 wage and salary income, but no self-employment income. Subsample E.2 includes men who report self-employment income (defined as net farm income, net income from non-farm businesses, and income from professional practice,

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[16] See, for instance, Herman P. Miller, 1966, *Income Distribution in the United States (A 1960 Census Monograph)*, Washington, D.C., U.S. Government Printing Office, Appendix A.

[17] Herriot and Spiers, p. 31.

Table 16.4 Relative Coverage of Self-Employment and Wage and Salary Earnings in Census and PSID. Samples Shown are Subsamples of Sample E: Male Heads of Households, Aged 25 to 64 in 1970, with Complete Data for Years of Education and 1969 Earnings. Self-Employment Earnings includes Net Farm Income, Net Income from Non-Farm Unincorporated Businesses, and Income from Professional Practice or Trade.

	Type of Income Reported							
	Wage and Salary Only		Self-Employment Earnings Only		Both Wages And S.E. Earnings		Neither	
	Sample E.1		Sample E.2		Sample E.3		Sample E.4	
	Census	PSID	Census	PSID	Census	PSID	Census	PSID
Weighted N	26551	1840	2760	187	1698	221	1540	90
Percent of Total	81.6%	78. 7%	8.5%	8.0%	5.2%	9.5%	4.7%	3.8%
Mean Wage and Salary	9444.91	9898.53	0	0	10121.97	9125.56	0	0
Wages S.D.	6179.49	5831. 67			8826.63	6766.08		
Correlation of Wages and Educ.	.36092	.46026			.32012	.43367		
Regression Coefficient Wages	637.59	749.46			792.33	890.74		
Mean Self-Employment Earnings	0	0	10461.43	11482.72	4124.03	2391.67	0	0
Standard Deviation			11614.58	11789.19	7756.58	2977.37		
Correlation of S.E. Earnings and Educ.			.42887	.34858	.16210	.09676		
Unstd. Regression Coefficient			1380.54	1156.47	352.57	111.21		
Mean 1969 Earnings	9444.91	9898.53	10461.43	11482.72	14155.65	11476.44	0	1124.01
S.D. Earnings	6179.49	5831.67	11614.58	11789.19	12357.85	6947.34		4027.50
Mean 1969 Personal Income	9793.08	10453.76	11031.47	12753.76	14739.78	11973.74	2613.90	3774.41
S.D. Income	6559.08	6280.57	12282.18	12810.81	12837.51	7248.43	NA	4068.58
Mean Years of Education	11.6180	11.7110	11.2361	11.4147	12.3441	12.7182	9.0424	8.6997
S.D. Education	3.4980	3.5814	3.6081	3.5535	3.5662	3.3824	NA	3.5044
Mean Age	42.6819	42.3571	47.1373	46.5467	44.1148	40.2061	53.5936	54.4543

partnership, or trade) or self-employment losses, but zero wages and salaries. Subsample E.3 includes men who report both kinds of earnings, and Subsample E.4, men who report neither wages and salaries nor self-employment earnings.

In order to divide PSID earnings into their component parts, I had to make several unsatisfactory assumptions about the possible combinations of earnings components. PSID provides only two of its earnings variables in dollar amounts: the overall earnings total (recomputed from Labor Income, see Part 2), and a dollar figure for wages and salaries excluding bonuses, overtime or commissions. Other earnings variables are coded in nine broad categories. The PSID earnings question solicits information about income from "farming or market gardening, roomers or boarders," not a separate component in the Census (see Table 16.1). I assigned a respondent to Subsample E.1 if his (narrowly defined) wages and salaries were greater than zero and the category variables indicating net farm income, net unincorporated business income, and income from professional practice or trade all equalled zero, whether or not he had income from farming or market gardening, etc. I assigned the entire earnings total for respondents in Subsample E.1 to Wage and Salary Earnings. I assigned respondents to Subsample E.2 by a similar scheme and assigned their entire earnings total to self-employment, whether or not the "bonuses, overtime, or commissions" indicator was zero. In dividing up earnings in Subsample E.3, I made the assumption that if a respondent had wages and self-employment income, he would not have bonuses, etc. as well. So I took the narrowly defined wage variable as Wage and Salary Earnings and assigned the rest to Self-Employment Earnings. These assignments and assumptions lead to certain ambiguities in the results. In particular, the positive earnings total in Subsample E.4 for men who report zero wages and zero net farm, zero net business and zero professional income. About seven of these 90 men have non-zero bonuses, etc., or non-zero farming and market gardening, etc., and 70 are genuine zero earners, but the source of earnings for the other 13 is a mystery to me. The difficulties in apportioning the components of PSID earnings must be kept in mind as we compare the PSID and Census results.

While a slightly smaller proportion of PSID respondents than Census respondents report wages as their only source of earnings (Sample E.1), a total of 88.2 percent of the PSID report non-zero wages compared to 86.8 percent in the Census (add E.1 and E.3). The mean Wage and Salary Earnings for those who report earnings is $9816 in the PSID and $9486 in the Census, or a difference of $330, about the same as for mean 1969 Earnings in Sample F (Table 3).[18/]
Contrary to expectation, then, the differences in PSID and Census earnings totals have at least as much to do with the reporting of wage and salary income as with self-employment income. Table 4 shows that wage and salary income is also the source of the higher correlations and regression coefficients for PSID earnings.

A total of 17.4 percent of the PSID sample report non-zero Self-Employment Earnings as do 13.7 percent of the Census (add Samples E.2 and E.3). The mean amount of Self-Employment Earnings reported is less in the PSID ($6552) than in the Census ($8048). But because more PSID

[18/] The PSID wage mean is biased upward slightly by the inclusion of income from farming and market gardening, roomers and boarders, for about 2 percent of the 1840 cases in Sample E.1. But the mean is biased down again by the assignment of income from bonuses, overtime and commissions to Self-Employment Earnings for 8.4 percent of the 221 cases in Sample E.3

respondents report non-wage earnings, mean Self-Employment Earnings for the whole PSID sample is $1140 compared to the Census $1103. The 3 percent difference between the two samples in self-employment income nicely matches the 3 percent difference in wages and salaries.

In contrast to results for Earnings, Census Self-Employment Earnings shows higher correlations with education than does PSID. The correlation for PSID Self-Employment Earnings in Sample E.3 is biased down because of errors introduced when I used a categorized income amount to re-estimate the Earnings of about half of these men (see Part 2). Even so, Census self-employment Earnings reports appear to be more predictable than PSID reports.

One possible assessment of Table 16.4 is that the differences between the Census and PSID are probably due to non-reporting, rather than under-reporting, and non-reporting of wages and salaries as well as self-employment income. The most striking differences between Census and PSID results are for Sample E.3, men who report both kinds of earnings. PSID Sample E.3 is significantly younger than its Census counterpart and evidently contains a relatively larger number of men with small amounts of income from self-employment. In the Census-CPS match, men who reported wages to the CPS but failed to report them to the Census had a mean of $3350. The figure for self-employment earnings was about $3310.[19] Assuming that 1.4 percent of the Census sample failed to report wages and salaries and 3.7 percent failed to report self-employment earnings (from the differences in PSID and Census percentages) and using the CPS means given above, I calculate that the CPS earnings mean could be biased downward by something like $170. Most of the assumed non-reporting would be on the part of men who reported wages but failed to report their self-employment earnings, or vice versa.

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[19] My calculations are from U.S. Bureau of the Census, Accuracy of Data, etc., 1975, Tables 49, 50 and 51.

An alternative **explanation is that** real sampling differences account for the differences in the proportion reporting various kinds of earnings in the two surveys. It could be that Census sampling procedures locate a group of men with low wage incomes who completely escape the PSID net. (My comparison of education levels for the two surveys supports this theory, c.f. Part 3.) In the next section I will compare the PSID and the NLS, two panel studies, to see if the PSID panel design can be invoked as an explanation for the PSID-Census discrepancies.

6: Problems of Panel Designs: A Comparison of the PSID and NLS Surveys

In panel designs, where interviewers return to the same respondents year after year, the problem of initial nonresponse is exacerbated by yearly attrition from the sample. Respondents dropping out of the sample are not necessarily random. Longitudinal designs tend to retain respondents on the basis of stability; highly mobile or unstable families are difficult to trace from year to year. In this section I will compare results from the PSID to results from another panel study, the NLS Mature Men, to see if there are similarities that can be attributed to the common problems of panel designs.

The NLS is particularly interesting in the context of my comparison between the Census and PSID. Although interview preparation, coding, data analysis, and published reports on the NLS have been the responsibility of the Center for Human Resource Research at Ohio State University, (and the survey usually goes by the name of NLS director Herbert Parnes), the Bureau of the Census designed and drew the sample, conducted the field work, and carried out the initial data processing.[20]

A basic limitation of the NLS sample I am using is that it only covers men who were between the ages of 45 and 59 in 1966, the first year of the survey.

To get the NLS sample, the Census Bureau took a preliminary, multi-stage, national probability sample of 35,360 housing units in early 1966. From the list of occupants they selected a sample of 5,518 men between the ages of 45 and 59. They stratified the sample by race within localities and over-sampled blacks to get a subsample large enough for separate analysis. The Bureau got usable interviews from 5,020 men or 91 percent of the original sample during the spring and summer of 1966. They weighted the cases to adjust for the systematic over-sampling of blacks. They also applied weights to compensate for nonresponse that would make the sample totals match the 1960 Census percentages for regions of the country and urban or rural residence. The NLS was not reweighted in subsequent years.

The NLS Earnings questions are closely parallel to the three Census Earnings questions. The only differences are the omission of the term "bonuses" from the question on wages and salaries and the phrasing of the net farm income question in terms of the family instead of the individual. Questions on other income components in the NLS are considerably more detailed than the Census questions. They ask for fourteen different totals instead of three. The list of specific probes for income components in the NLS pretty much duplicates that in the PSID. All NLS income components are coded in dollar amounts to $50,000.

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[20] My comments on technical specifications of the NLS are derived from The National Longitudinal Surveys Handbook, Center for Human Resource Research, The Ohio State University, 1973. There are four NLS samples, a young male cohort, a young female cohort, and a middle-aged female sample in addition to the sample of mature men used here.

Table 16.5 presents earnings and education results from the 1969 waves of the PSID and NLS surveys. The PSID survey has been restricted to men between the ages of 48 and 62 in 1969. The samples shown are analogous to samples E and F in earlier tables. The first pair of columns includes only heads of households with complete data on Age, Sex, Household Relationship, Years of Education and 1968 Earnings. The second pair of columns has similar restrictions but omits men with zero or negative 1968 Earnings. The PSID sample uses 1968 weights . It estimates Earnings using a procedure that slightly over-estimates some self-employment earnings.

As was the case for the Census-PSID comparison, the PSID sample has more education than the NLS. Because of the small sample size of the PSID, however, the differences are probably not statistically significant. Mean Earnings for the two surveys are practically identical. Again mirroring the Census-PSID comparison, the standard deviations of Earnings differ significantly (F larger than 1.2). In a reversal of the pattern, the PSID has smaller unstandardized regression coefficients although the differences are not statistically significant. The PSID correlation coefficients and R^2 totals exceed the NLS by almost as much as they exceeded the Census results, but the differences, again, are not significant. The totals are not shown in Table 5, but the PSID finds about $300 more unearned income per person than the NLS. In sum, the NLS-PSID comparison reveals some familiar elements--higher PSID education, lower PSID standard deviations of earnings, higher

Table 16.5

Comparison of Earnings and Education Results from the PSID and NLS. Means, Standard Deviations and Regressions with Years of Education for 1968 Earnings and Ln Earnings. Samples are Male Heads of Households, Aged 48 to 62 in 1969, with Complete Data on Age, Sex, Household Relationship, Years of Education and 1968 Earnings. (The second sample omits men with zero or negative earnings in 1968.)

			Earnings Greater Than Zero	
	NLS	PSID	NLS	PSID
Unweighted N	4041	690	3728	629
Education Mean	10.1117	10.4379	10.2338	10.5955
Education S.D.	3.7017	3.8350	3.6546	3.8139
Earnings Mean	8504.76	8492.43	9174.53	9103.77
Earnings S.D.	8141.07	7377.75	8062.53	7264.07
Correlation	.41515	.45595	.42016	.44705
Reg. Coef.	913.05	877.16	926.93	851.48
S.E.	31.49	65.32	32.80	68.09
R^2	.17235	.20789	.17653	.19985
Ln Earning Mean			8.8432	8.8562
Ln Earning S.D.			.9231	.8393
Correlation			.40523	.47570
Regr. Coef.			.10236	.10468
S.E.			.00378	.00774
R^2			.16421	.22629

PSID correlations--and some novelties--closely matched earnings means, insignificantly lower PSID regression coefficients. These last two deviations from the Census-PSID comparison pattern are more apparent than real, however. When I compared five-year cohorts from the Census and PSID, I found that the PSID had lower mean earnings than the Census for cohorts aged 50 to 54 and 55 to 59 in 1970. PSID regression coefficients were also lower than the Census coefficients for the 50 to 54 and 60 to 64 year-old cohorts. These cohorts include most of the PSID respondents in the PSID-NLS comparison. The only significant difference between PSID and NLS is the variance of earnings.

The earnings reliability figures shed no light on the sources of the discrepancies between the NLS and PSID results. The estimated reliability of the earnings reports of PSID men over 45 is slightly lower than for the sample as a whole, about 0.86 for 1968 Earnings. The NLS provides earnings reports for 1965, 1966 and 1968 Earnings, so I can calculate reliability for the middle year of the three by the use of a simplex model and the intercorrelations of the reports. The resulting reliability estimate for NLS 1966 Earnings is at least 0.90. Yet comparison of standard deviations and R^2's for the two surveys suggests that, if errors were mainly random and the surveys were sampling the same population, PSID reliability should be higher than NLS reliability rather than lower. Only two explanations seem possible: a) the two surveys are not, in fact, sampling the same population, or b) interview techniques, question wording, editing, data processing, or some other unnamed "survey organization" effects beyond the reach of the secondary analyst are responsible for the differences. The answer is probably some combination of the two.

I conclude that the differences between the Census and PSID cannot be attributed to problems common to all panel designs. There is still reason to believe that some of the **anomalies** of the PSID results can be traced to specific PSID sampling problems. The PSID tape is set up in such a way that one can only process cases from families that still remained in the sample in 1972, the fifth year of the panel. Thus the detrimental effects of year to year attrition cannot be avoided by using an earlier panel year. An important subsample of the PSID, the low-income respondents from the OEO sample, is subject to seven years rather than five of sample attrition. This subsample lost cases in the two years the Census Bureau followed it and then lost even more in the transition to the PSID. The nonresponse rate for the low-income group in the initial PSID interview (1968) was exceptionally high because of respondent refusal to sign release forms, OEO's failure to supply some addresses, and SRC difficulties in locating some of the respondents for which it did have release forms and addresses.[21/]

The PSID weights were intended to compensate for nonresponse, but the use of weights depends on the assumption that those who do respond from a given neighborhood can be taken as representative of those who are not at home or refuse to be interviewed.[21A/] **This is undoubtedly a** faulty assumption, because most neighborhoods are heterogeneous to some degree, and nonrespondents are likely to be drawn from the ranks of the highly mobile and economically marginal in any setting. SRC reports, for instance, that apartment dwellers had higher rates of nonresponse

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[21/] See SRC, 1972, pp. 9-33.

[21A/] This argument applies only to use of weights to compensate for nonresponse in the first year of the PSID. Subsequent reweighting was based on other nonrespondent characteristics in addition to location.

in the PSID than homeowners. The difficulties of nonresponse and reweighting for the low-income OEO subsample of the PSID may be reflected in the results for PSID non-whites (see Part 10 of this chapter), which are especially divergent from Census results.

.7 Coding, Definitional Differences and Functional Forms

In this section and the ones that follow, I shift my primary emphasis from the question of whether the Census and PSID can be made to yield similar results to the question of whether procedural treatments have consistent results in the Census and PSID and can, thus, be expected to affect other surveys in the same way. This section begins by asking whether my substitution of a standardized coding for education and earnings introduces any distortions into my results. It goes on to discuss the ways in which the choice of income definitions, choice of functional forms, and use of broad-category income codes affect outcomes from the Census and PSID.

Table 16.6 demonstrates that my use of a six-category code for Years of Education has no important impact on the earnings and education statistics for the Census or the PSID or the comparison of the two surveys. The columns labelled Original Coding in Table 16.6 report statistics based on the Census education coding used by Bartlett and Jencks (Appendix A) and the PSID education coding used by Mueser (Appendix D). The remainder of the table makes use of a standard six-category code. The categories are: 0 to 5 years of education (estimated mean 3.0 years), 6 to 8 years (mean 7.5), 9 to 11 years (mean 10.0), 12 years, 13 to 15 years (mean 14.0), and 16 years and over (mean 17.25). I have used a standard coding for the 1969 Earnings variable throughout the table.

Table 16.6: Effects of Standardizing the Education Codes: Comparison of Means, Standard Deviations and Correlations (with 1969 Earnings in Standard Form) for the Original Codes and Standard Code of Years of Education. Sample E: Census and PSID Male Heads of Households, 25 to 64 Years Old in 1970, with Complete Data. Sample F: Additional Restriction to Respondents with Earnings Greater than Zero. (Earnings means and standard deviations are in Tables 8 and 9.).

	SAMPLE E				SAMPLE F			
	Original Coding of Years of Education		Standard Coding of Years of Education		Original Coding of Years of Education		Standard Coding of Education	
	Census	PSID	Census	PSID	Census	PSID	Census	PSID
Weighted N	32549	2338	32549	2338	30969	2268	30969	2268
Unweighted N		2349		2349		2255		2255
Education Mean	11.4813	11.5847	11.5017	11.6674	11.6084	11.6732	11.6248	11.7583
Education S.D.	3.6414	3.5156	3.5823	3.6182	3.5712	3.4744	3.5176	3.5790
Standard Error of Educ. Mean	.0202	.0726	.0199	.0747	.0203	.0732	.0200	.0754
Correlation with 1969 Earnings	.37599	.44099	.37020	.44089	.36243	.43035	.35639	.42990
Unstandardized Regression Coef.	772.66	854.15	773.32	829.72	746.45	827.17	745.17	802.13
Standard Error of Regression Coef.	10.56	35.97	10.76	34.86	10.91	36.45	11.10	35.49
R^2	.14137	.19448	.13705	.19438	.13136	.18520	.12701	.18481
Correlation with Ln Earnings					.37321	.45000	.37357	.44938
Unstandardized Regression Coef.					.07450	.08967	.07571	.08692
Standard Error of Regression Coef.					.00105	.00374	.00107	.00363
R^2					.13928	.20250	.13955	.20194

The six-category code appears to increase the difference between the Census and the PSID education means. This effect is due to a faulty choice of category means for the original PSID education code. The estimated average of two years beyond college graduation for men with advanced or graduate degrees was almost certainly too low. The proper estimate may be as high as 19 years of education for these men. The next-to-the-top PSID education category includes some men who have completed one or more years of graduate school in addition to the men with exactly 16 years of education, so the estimated mean of 16 in the original PSID code is a little low for this category, as well. What is happening is that the PSID sample is better educated than the Census sample, and the use of the standard education code makes this fact more obvious.

The shift to the six-category education code appears to reduce the difference between unstandardized regression coefficients for the two surveys, but, once again, the changes can be traced to my respecification of category means for the PSID education variable. Given certain reasonable assumptions, categorization of the adjacent values of an independent variable will not bias unstandardized regression coefficients, unless the category means used are inaccurate.[22] The Census regression coefficients are, thus, virtually unchanged by substitution of the standard education code. The mistaken estimation of category means for the original PSID education code had biased the regression coefficient up a little; the standard code eliminates that bias.

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[22] On categorization and bias, see Blalock, 1964, op. cit., and Michael T. Hannan and Leigh Burstein, 1974, "Estimation from Grouped Observations," American Sociological Review 39 (June): 374-392.

The six-category education code makes earnings-education correlation coefficients drop a little, because of the loss of some within-category variance for Years of Education. Since the loss of within-category variance is greater for the Census, Census correlation coefficients show more decline, making the difference between surveys a little larger than in the original comparison.[23]

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[23] The standard treatments of "grouping" (e.g. Blalock, 1964) presuppose the kind of aggregation that involves a change in the level of analysis, as, for instance, in calculating income and education correlations from means for Census tracts instead of individual reports, or the regression of mean test scores for school classes on the mean social backgrounds of the students. The kind of procedure I discuss in the text is perhaps better labelled "categorization" than "grouping." I substitute a single mean for several adjacent education responses, but do not change income responses. Thus, only the independent variable suffers any loss of within-category variance, and correlation coefficients are biased downward. Since $r = bs_x/s_y$, where r is the correlation coefficient, b is the unstandardized regression coefficient, and s_x and s_y are standard deviations of the independent and dependent variables, an increase in the ratio of s_x to s_y with b unchanged will bias r upward. If b and s_y are both unaffected, as in the case for my data, the reduction in s_x due to grouping will reduce r.

Table 16.7 presents means and standard deviations for standardized 1969 Earnings within each category of the original and standardized education variables.[24]

A few findings from the columns labelled Standardized Coding deserve a brief mention. The Census and PSID differ most in the earnings means for the lowest and the highest education categories. The lowest category is the only one for which the standard deviation of PSID earnings exceeds the Census. This shows the effects of differences in question wording (see above). The extra question on literacy in the PSID allowed SRC coders to treat as valid some responses that would otherwise have been missing data. This increased the number and heterogeneity of PSID respondents in the lowest education category. The only category for which the PSID has a lower earnings mean than the Census is the "9 to 11 years" category. For the highest education category the PSID mean is over $1000 larger than the Census mean, but the finding is of doubtful statistical significance, because of the small number of PSID respondents at this level.

Tables 16.8 through 16.12 explore differences in results caused by alternative income codings, transformations, and definitions. Tables 16.8 and 16.9 present Census and PSID 1969 Earnings results for Samples E and F, respectively. The difference between the two samples is that zero and negative earners are excluded from Sample F. The first few columns of Tables 16.8 and 16.9 answer

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[24] In the column that gives the original coding of the PSID education variable, I have shown the SRC distinction between literate and illiterate respondents with less than six years of education and between high school graduates with nonacademic training and others, even though I grouped these two categories together in the calculations labelled "original coding" in Table 6.

Table 16.7
1969 Earnings Means and Standard Deviations by Years of Education Categories for Education Coding Alternatives. Census and PSID Sample E. Category N in Parentheses Under Means.

Original Coding of Years of Education						Standardized Coding of Years of Education				
Census			PSID			Standard Coding (years)	Census		PSID	
Coding (years)	Mean	S.D.	Coding (years)	Mean	S.D.		Mean	S.D.	Mean	S.D.
All	9330 32549	7483	All	9838 2338	6809	All	9330 32549	7483	9838 2338	6809
0	4708 (213)	7363	0 to 5 (illiterate)	4822 (47)	7500					
1	3749 (67)	3715								
2	3347 (149)	2981								
3	3726 (283)	3427	0 to 5 (literate)	4994 (69)	3792	0 to 5	4270 (1603)	4250	4924 (116)	5575
4	4051 (388)	3482								
5	4903 (503)	3659								
6	5350 (900)	3756								
7	6178 (1182)	4935	6 to 8	6617 (381)	4268	6 to 8	6447 (5497)	5055	6617 (381)	4268
8	6832 (3415)	5337								
9	7444 (1970)	5018								
10	7788 (2364)	4592	9 to 11	7632 (394)	3961	9 to 11	7828 (6168)	4945	7632 (394)	3961
11	8307 (1834)	5240								
			12	9473 (468)	4094					
12	9229 (10155)	5696	12 + non-academic	10153 (231)	6564	12	9229 (10155)	5696	9698 (698)	5049
13	10400 (1395)	7672								
14	10909 (1812)	8157	13 to 15	11127 (358)	6192	13 to 15	10830 (3887)	8075	11127 (358)	6192
15	11518 (680)	8580								
16	13753 (2603)	9652	16 (BA)	14859 (243)	8333					
17	14585 (705)	9467				16 and over	14728 (5239)	11208	15734 (390)	9797
19 (18 or more)	16101 (1931)	13409	18 (graduate degree)	17177 (147)	11713					
Eta^2	.1491			.2160			.1438		.2108	

Table 16.8

Effects of Income Definitions and Coding: Means, Standard Deviations and Correlations for 1969 Earnings, Ln Earnings, and Earnings$^{1/3}$ with a Standardized Years of Education Variable. 1970 Census and 1970 PSID Male Heads of Households Aged 25 to 64 with Complete Data for Earnings, Education, Sex, Age, and Household Status. **(Sample E)**

	1969 Earnings: Original Code			1969 Earnings Standard Code		Ln Earnings (Standard)		Earnings$^{1/3}$ (Standard)	
	Census	PSID	PSID (new calculation) *	Census	PSID	Census	PSID	Census	PSID
Weighted N	32549	2338	2338	32549	2338	32599	2338	32549	2338
Unweighted N		2349	2349		2349		2349		2349
Mean	9334.07	9823.99	9803.18	9329.99	9838.39	8.5437	8.7612	19.4736	20.1709
Standard Deviation	7535.35	6870.92	6822.90	7483.27	6809.23	2.0513	1.6857	6.1564	5.5122
Standard Error of Mean	41.78	141.79	140.80	41.48	140.52	.01137	.03479	.03412	.11377
Correlation Coefficient	.36785	.43852	.44102	.37020	.44089	.26449	.30776	.38006	.44709
Unstandardized Regression Coef.	773.76	832.74	831.64	773.32	829.72	.15145	.14338	.65315	.68112
Standard Error of Regression Coef.	10.84	35.22	34.93	10.76	34.86	.00306	.00915	.00881	.02813
R^2	.13531	.19230	.19450	.13705	.19438	.06995	.09472	.14445	.19989
Standard Deviation of Residuals	7007.11	6175.04	6124.83	6951.71	6113.01	1.97826	1.60424	5.69447	4.93165

	Census	PSID
Education Mean for Sample E	11.5017	11.6674
Education Standard Deviation	3.5823	3.6182

* This column contains figures based on my slightly modified procedure for estimating PSID earnings. (See text) The column to the left of this one contains Mueser's estimate. (See Appendix D) All subsequent PSID columns are based on my modified estimate.

Table 16.9

Effects of Removing Zero Earners: Means, Standard Deviations and Correlations for 1969 Earnings, Ln Earnings, and Earnings with a Standardized Years of Education Variable. 1970 Census and 1970 PSID Male Heads of Households Aged 25-64 in 1970 with Positive Earnings and with Complete Data for Earnings, Education, Sex, Age, and Household Status. (Sample F)

	1969 Earnings: Original Code			1969 Earnings Standard Code		Ln Earnings Standard Code		Earnings$^{1/3}$ Standard Code	
	Census	PSID	PSID (new calculation) *	Census	PSID	Census	PSID	Census	PSID
Weighted N	30969	2268	2268	30969	2268	30969	2268	30969	2268
Unweighted N		2255	2255		2255		2255		2255
Mean	9813.89	10132.02	10110.56	9809.59	10146.88	8.9796	9.0324	20.4671	20.7952
Standard Deviation	7410.38	6743.60	6694.14	7354.98	6678.03	.7129	.6923	4.4158	4.2821
Standard Error of Mean	42.11	142.04	141.00	41.80	140.66	.00405	.01458	.02093	.09019
Correlation Coefficient	.35393	.42746	.43008	.35639	.42990	.37357	.44938	.40487	.47638
Unstandardized Regression Coef.	745.61	805.43	804.41	745.17	802.13	.07571	.08692	.50825	.56996
Standard Error of Regression Coef.	11.20	35.89	35.57	11.10	35.49	.00107	.00364	.00652	.02216
R^2	.12527	.18272	.18497	.12701	.18481	.13955	.20194	.16392	.22694
Standard Deviation of Residuals	6930.82	6096.45	6044.74	6872.15	6030.78	.66128	.61860	4.03777	3.76587

	Census	PSID
Education mean for Sample F	11.6248	11.7583
Education standard deviation	3.5176	3.5790

* This column contains figures based on my slightly modified procedure for estimating PSID earnings. (See text) The column to the left of this one contains Mueser's estimate. (See Appendix D) All subsequent PSID columns are based on my modified estimate.

the question of whether my standardization of the income codes for the Census and PSID introduces any distortion into the analysis. Columns labelled "Original Code" use the same codes as Bartlett and Jencks used in Appendix A and Mueser in Appendix D. The only nontrivial difference arising from my use of a standardized code is the small contraction of the standard deviation of earnings for both surveys caused by my substitution of a single estimated mean for a range of values over $50,000. (See Part 2 above.)

I have included a column in Tables 16.8 and 16.9 to show the effects of my modification of Mueser's algorithm for reconstructing PSID earnings. The reason for modifying the calculation of PSID earnings was to get a better estimate of the earnings of men who had both self-employment and wage earnings and of men whose self-employment losses had been excluded from the previous earnings calculation. These men constitute no more than 4 percent of the PSID respondents. I decreased these men's estimated 1969 Earnings by an average of about $550. This lowers the overall sample mean by $21. (Compare columns labelled "PSID original code" and "PSID new calculation" in Tables 16.8 and 16.9.) The changes in the standard deviations and correlations for the overall sample a[illegible]ally trivial. The modified earnings calculation only makes an important difference for subsamples that isolate self-employmed respondents.

To this point I have been dealing only with income in dollars. Elsewhere in the book, however, we have reported equations that make use of two income transformations: the natural logarithm and the cube root. Tables 16.8 and 16.9 show what happens when I apply these transformations to 1969 Earnings for the samples I am discussing in this chapter. The use of a logarithmic transformation is entwined with another theoretical and methodological issue: what to do about men in the sample who report zero earnings or losses for the year in question. The theoretical question is what it means to include men who did not work in the previous year in estimates of economic returns to education. Lack of education may, of course, be one reason why some men do not work. But if we want to assess the effect of education on labor force participation, it is better to do this directly than to merge the issue of participation with the issue of earnings in a single analysis. This suggests dropping men without earnings from analyses of the determinants of earnings. If these men are included, one must decide what logarithmic value to give them. The logarithm of zero is defined as $-\infty$. Assigning this value will lead to nonsense results. Researchers who want to employ the logarithmic transformation while retaining men without earnings often assign a logarithm of zero to cases with zero earnings or income. (I have done this in Table 16.8.) This is mathematically equivalent to assigning these men earnings of $1.00 for the year--hardly a tenable theoretical proposition. Another expedient is the assignment of $0.01 for zero earnings.[25/] This assignment approximates zero more closely and has a finite logarithm.

[25/] Leonard Weiss and Jeffrey G. Williamson, 1975, "Black Education, Earnings and Interregional Migration: Even Newer Evidence," American Economic Review 65 (March):241-244, claim that Thurow used 0.01 in an earnings equation. Their claim is probably mistaken, since Thurow's data (income medians for age-education groups) should not have zeros. Weiss' and Williamson's confusion apparently arose from Thurow's use of the log of 0.01 years to estimate the log of zero education. See Lester C. Thurow, 1969, Poverty and Discrimination, Washington, D.C., Brookings Institution, Appendix J.

Yet another alternative is to assign $100, on the theory that zero reporters are really underreporting small amounts of income. Table 16.10 illustrates the problems one faces with any arbitrary assignment scheme for the logarithm of zero earnings. I have used the same data for all three columns, but assigned the logarithm of 0.01 to zero and negative earners in the first column, the logarithm of 1.0, in the second column, and the logarithm of 100 to all earners of under $100 in the third column. Since the logarithmic transformation compresses the upper end of the income scale, and exaggerates the differences at the lower end of the income scale, the zero and negative earners are extreme outliers under any assignment scheme. The arbitrary value one has assigned to them has a drastic effect on the results. Variance explained for the same data ranges from 5.3 percent to 11.3 percent. The unstandardized regression coefficients of Ln Earnings on education range from 0.19 to 0.11. The differences are greater than twice the standard errors of the coefficients.

Elsewhere in this book we have simply omitted zero and negative earners from our analyses. This decision has a fairly small impact on the statistics for untransformed dollars earnings. In general the omission of zero and negative earners means a drop in the size of correlations and unstandardized regression coefficients for earnings with education. (See Part 3 on basic sample restrictions.)

Other chapters in this book focus on the comparison of results from a number of other surveys with the results from the 1962 Occupational Changes in a Generation survey. One complicating factor in the comparisons that involve the OCG is that the income measure available for that survey is total personal income rather than respondent's earnings. The

Table 16.10

The Arbitrary Assignment of Values for the Logarithm of Zero Earnings-Means, Standard Deviations, and Bivariate Regressions with Years of Education for Three Versions of Ln Earnings, 1970 Census (Sample E)

	Version I Log of Zero= Ln (0.01)	Version II Log of Zero= Ln (1.)=0	Version III Log of Zero= Ln (100.)
Weighted N	32549	32549	32549
Ln Earnings Mean	8.3202	8.5437	8.7681
Ln Earnings Std. Deviation	3.0012	2.0513	1.1659
Standard Error of Mean	.0166	.0114	.0065
Correlation Coefficient	.23097	.26449	.33591
Unstandardized Regression Coef.	.19350	.15145	.10932
Standard Error of Regres. Coef.	.00452	.00306	.00170
R^2	.05335	.06995	.11284
S.D. of Residuals	2.9201	1.9783	1.0982

shift from earnings to total income entails several other subtle changes having to do with sample restrictions. More respondents have missing data on total income than on earnings alone. On the other hand, fewer respondents have zero total income than zero earnings. In addition, the OCG income variable was coded into rather broad categories, and, while zero-income respondents had a separate category, there was no way of distinguishing between respondents with negative income and respondents in the lowest positive income category.

Tables 16.11 and 16.12 apply this OCG set of definitional, sampling and coding changes to the Census and PSID income data. A comparison of Sample G in Table 16.11 to Sample E in Table 16.8 shows the combined effect of change in the income definition and elimination of respondents with missing data for unearned income components. From the differences between the means for income and earnings it appears that the PSID is more successful in "finding" unearned income than the Census. Not only is the income-earnings difference larger for the PSID, it is larger in percentage terms, as well. The unearned income found by the PSID lowers the correlation with education by about 0.02. Inclusion of unearned income has virtually no effect on the Census correlation, but it increases the unstandardized regression coefficient by $809 - $773 = $36, about 3 times the standard error. The PSID and Census are more consistent when it comes to changes between cube-root earnings and cube-root income. The change to income increases the mean cube-root by about 0.8, decreases the standard deviation, increases the correlation by about 0.035, and decreases the unstandardized regression coefficient for both surveys.

I have just been dealing with samples that include respondents with earnings or income zero totals. Exclusion of these zeros makes

Table 16.11

Effects of Income Definitions and Coding: Means, Standard Deviations, and Correlations with Education for 1969 Personal Income, Ln Income, and Income$^{1/3}$ and the same variables in a typical CPS-category coding. Samples Are 1970 Census and PSID Sample E with the additional deletion of respondents with any missing data for asset or transfer income. (Sample G)

	1969 Income Original Code		1969 Income Standard Code		1969 Income CPS Code		Ln Income Standard Code	
	Census	PSID	Census	PSID	Census	PSID	Census	PSID
Weighted N	31171	2297	31171	2297	31171	2297	31171	2297
Unweighted N		2313		2313		2313		2313
Mean	9864.73	10406.60	9835.53	10443.23	10080.09	10612.69	8.8897	9.0531
S.D.	8119.28	7087.27	7789.16	7151.21	7782.26	7226.23	1.1399	.7591
S.E. of Mean	45.99	147.36	41.19	148.73	44.08	150.29	.0065	.0158
Corr.	.36066	.42535	.37090	.42211	.37456	.43043	.30733	.41838
Unstd. Reg.Coef.	819.63	832.72	808.65	833.83	815.89	859.18	.09806	.08773
S.E. of Reg.Coef.	12.01	36.86	11.47	37.25	11.44	37.47	.00172	.00396
R^2	.13007	.18092	.13757	.17817	.14029	.18527	.09445	.17504
S.D. of Residuals	7572.97	6415.58	7233.70	6484.31	7215.87	6524.00	1.08478	.68965

Table 16.11 continued

	Ln Income CPS Code		Income$^{1/3}$ Standard Code		Income$^{1/3}$ CPS Code	
	Census	PSID	Census	PSID	Census	PSID
Weighted N	31171	2297	31171	2297	31171	2297
Unweighted N		2313		2313		2313
Mean	8.9283	9.0778	20.2823	20.9779	20.4808	21.0976
S.D.	1.0887	.6863	4.9922	4.3627	4.9517	4.3416
S.E. of Mean	.00617	.01427	.02878	.09071	.02805	.09029
Corr.	.31635	.45721	.41528	.48165	.41744	.48807
Unstd. Reg.Coef.	.09640	.08667	.58029	.58044	.57857	.58533
S.E. of Reg. Coef.	.00164	.00351	.00720	.02197	.00713	.02177
R^2	.10008	.20904	.17246	.23198	.17426	.23821
S.D. of Residuals	1.03277	.61049	4.54144	3.82416	4.49967	3.79016

	Census	PSID
Education Mean for this sample	11.5104	11.6346
Education Std. Dev.	3.5727	3.6202

Table 16.12

Effects of Removing Respondents with Zero Income: Means, Standard Deviations and Correlations with Education for 1969 Personal Income, Ln Income, and Income$^{1/3}$ and the same variables in a typical CPS-category coding. Samples Are . 1970 Census and PSID Sample E with the additional deletion of respondents with zero income and missing data for asset or transfer income. (Sample H)

	1969 Income Original Code		1969 Income Standard Code		1969 Income CPS Code		Ln Income Standard Code	
	Census	PSID	Census	PSID	Census	PSID	Census	PSID
Weighted N	30909	2295	30909	2295	30909	2295	30909	2295
Unweighted N		2310		2310		2310		2310
Mean	9948.35	10414.11	9918.90	10450.77	10165.53	10620.35	8.9650	9.0596
S.D.	8102.45	7084.31	7769.07	7148.28	7759.41	7223.21	.7968	.7194
S.E. of Mean	46.09	147.43	44.19	148.76	44.14	150.33	.00453	.01497
Corr.	.35850	.42507	.36879	.42182	.37243	.43015	.37737	.43544
Unstd. Reg.Coef.	816.22	831.76	805.10	832.87	812.05	858.21	.08449	.08652
S.E. of Reg. Coef.	12.09	36.87	11.54	37.26	11.51	37.49	.00118	.00372
R^2	.12852	.18068	.13601	.17793	.13871	.18503	.14241	.18961
S.D. of Residuals	7564.01	6413.85	7221.57	6482.61	7201.30	6522.23	.73790	.64772

Table 16.12 continued

	Ln Income CPS Code		Income$^{1/3}$ Standard Code		Income$^{1/3}$ CPS Code	
	Census	PSID	Census	PSID	Census	PSID
Weighted N	30904	2295	30909	2295	30909	2295
Unweighted N		2310		2310		2310
Mean	9.0040	9.0844	20.4543	20.9930	20.6544	21.1129
S.D.	.7168	.6417	4.6494	4.3277	4.5979	4.3060
S.E. of Mean	.00408	.01335	.02645	.09006	.02615	.08961
Corr.	.41079	.48215	.42494	.48341	.42808	.48996
Unstd. Reg.Coef.	.08274	.08546	.55517	.57786	.55309	.58274
S.E. of Reg.Coef.	.00104	.00323	.00673	.02178	.00664	.02158
R^2	.16875	.23247	.18058	.23369	.18326	.24006
S.D. of Residuals	.65353	.56231	4.20877	3.78927	4.15540	3.75452

	Census	PSID
Education Mean for this sample	11.5318	11.6364
Education Std. Dev.	3.5587	3.6204

the comparison more complicated. Only a small minority of the men in these surveys that report zero earnings also report that they had no unearned income. Practically none of the PSID respondents reports zero income; for the Census, it is slightly under one percent of the total. (See Table 16.2.) Using zero income instead of zero earnings as the exclusion criterion therefore adds a number of zero earners who typically have very low incomes. Tables 16.9 and 16.12, showing Samples F and H, can be used to compare the combined effects of substituting income for earnings, deletion of income allocations for deletion of earnings allocations, and deletion of zero income respondents for deletion of zero and negative earners. This combined treatment has inconsistent effects on the two surveys, especially for the log transformation, so it would be unwise to try to generalize from these data to other surveys. In any case, none of the changes is very large.

The OCG measure of income differs in one additional way from the Census and PSID. OCG income is a bracketed variable with rather wide income categories--five hundred dollars wide up to $4.999 and a thousand dollars wide or wider for the rest of the income continuum. I intended to test the effects of using this category system, but found that direct application of the OCG code to 1969 income distributions would have resulted in a misleading test, since income means rose by more than 50 percent in the 8 years separating the OCG from the Census and PSID. Simply multiplying the OCG categories by a constant factor to account for growth in incomes was not a good solution, since it would have obscured an important feature of the OCG code: that round numbers serve as lower limits for each category (except the lowest).

Round numbers show up with such frequency in income distributions that midpoints seriously overestimate the means of categories with round lower limits and "99" upper limits.

OCG income figures were gathered as part of a Current Population survey (CPS) by the Bureau of the Census. The OCG categories are similar to the income-reporting categories that appear in CPS publications of the period. For my test categorization I used a CPS categorization of the type that appears in CPS reports from around 1970, slightly modified so that the total number of categories will be about the same as for OCG. (I also use this categorization for reporting the income frequencies in Table 16.2.) I took the midpoints of the categories up to $14,999 as the means of these categories. I chose round numbers below the midpoints as estimates of the means of the remaining categories (i.e. 17,000; 22,000; 33,000; and 70,000). Jackson followed a similar procedure in assigning means to the OCG code (see Appendix B.). Table 16.12 demonstrates that taking the midpoints as category means systematically overestimates the real category means of reported income for both Census and PSID. For categories from $1000 and $9999, the true means fall below the midpoints by an average of $134 in the Census and $65 in the PSID. The explanation is that responses that are multiples of $1000 are especially popular income responses, either because of a greater incidence of incomes set to exactly those figures, or because respondents round off their answers. If other responses were distributed evenly across the categories, about 10 to 15 percent of the PSID respondents and about 30 percent of the Census respondents in the 1000 to 9999 range would have to give a response exactly divisible by 1000 to deflect the means by the amounts shown. The PSID evidently succeeds in getting fewer rounded answers, possibly because of its use of interviewers rather than self-report forms, or because of its more complex income categories, or because returning year after year to the same respondents develops the rapport that prompts them to give more detailed answers.[26]

It appears from these data that there is a hierarchy of degrees of popularity for rounded responses. Multiples of 1000 are more popular than other multiples of 100. $5000 and probably $10,000 are especially popular responses. (In both the Census and PSID the means for the 5000 to 5999 category have comparatively large deflections.) The zero response is probably another popular target for rounding off responses. If the format of the Census survey encouraged rounding off on the part of the respondents, this could be part of the explanation for the larger number of zero earnings respondents in the Census than in the PSID.

The CPS categorization inflates overall income means by about $200 (See columns labelled CPS code in Tables 16.11 and 16.12.[27] CPS coding has no other important effects on statistics on dollar income or its regression on education. Though treatment of the open-ended category is identical in the Standard and CPS codes, the systematic overestimation of the means for the other higher income brackets in the CPS code apparently inflates standard deviations and unstandardized regression coefficients enough to counteract any downward bias due to the categorization itself. Regression coefficients for the CPS code are also a little higher but none of these differences is as large as its standard error.

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[26] Even for categories only $100 wide the use of midpoints as estimated category means leads to an upward bias in the mean for the distribution. Applying my "standard coding" to the raw dollar reports of PSID earnings caused about a $35 increase in the PSID mean. Compare "new calculation" and "Standard Code" columns in Tables 16.8 and 16.9.

[27] The Bureau of the Census reports similar findings. See U.S. Bureau of the Census, 1970, Current Population Reports, Series P-60, No. 74, "Annual Mean Income and Educational Attainment of Men in the United States for Selected Years, 1956 to 1968," U.S. Government Printing Office, Washington, D.C., pp 20-22. The $200 estimate depends only on category width and propensity to round off, so the size of the bias in percentage terms is greater for surveys taken in earlier years.

For log income (Table 16.12) CPS codes have a greater range of consequences. Besides increasing the mean (by an amount a good deal less than the coefficient for a year of education), CPS coding decreases the standard deviation of Ln Income. This is because the lower limit of the CPS distribution is $500 instead of $50. In logs, this is equivalent to reducing the upper limit from, say, $250,000 to $25,000. Rescaling the lowest income responses raises R^2 by about 0.03 for the Census and 0.04 for the PSID regressions for Ln Income. But because rescaling lowers the variance of the dependent variable, it lowers the unstandardized regression coefficients as well.

Putting together all of the ways in which OCG income differs from Census categorized earnings (Comparison of Sample F in Table 16.9 to CPS columns of Table 16.12 Sample H), I find that the substitution of broadly categorized income and income-related exclusions for earnings results in a substantial increase in the dollar mean and standard deviation ($300 to $500), some increase in the unstandardized regression coefficient of education, and only slight (0.01 or less) increases in variance explained. For the log transformation, the substitution of OCG-type income brings about noticeable upward shift in means and in R^2, a reduction in the standard deviation, and inconsistent changes in the unstandardized regression coefficients.

On the basis of these trends, I would expect an uncategorized measure of OCG earnings to differ from OCG income in the following ways: a) mean and standard deviation of dollar earnings would be about 5.5 percent lower; b) the correlation of education with dollar earnings would be about 0.01 lower; c) the mean of Ln Earnings would be about 0.03 lower; d) the standard deviation of Ln Earnings would be the same; e) the correlation of education with Ln Earnings would be about 0.03 lower. These changes imply slightly lower regression coefficients, as well.

8 Accounting Periods and the Predictability of Earnings

Some early project analyses showed that, for restricted samples from the PSID, education could explain a much larger percentage of the variance in total earnings over four or five years than in earnings for a single year (see Coleman and Rainwater, forthcoming). A plausible explanation for the result was that some men (e.g. self-employed men, salesmen on commission, workers subject to layoffs) have earnings that vary a good deal from year to year. For these men an average from several years provides a more reasonable indicator of "earning power" and standard of living than earnings for any single year. Taking an average for several years also reduces the effects of random measurement error.

Averaging requires certain sample restrictions. Some respondents in a one-year sample will die or drop out of the panel in subsequent years. Others will be lost because of restrictions for missing data or zero earnings. These sample restrictions may affect the predictability of earnings even for a single year, since the least stable respondents are most likely to be lost.

Table 16.13 shows the impact of the use of a five-year accounting period

Table 16.13

The Five-Year Accounting Period for Earnings. Means, Standard Deviations, and Education Regression Statistics for 1969 Earnings, Ln Earnings, 1967-71 Average Earnings (in 1969 dollars) and Ln Average Earnings.

Sample I: PSID Male Heads of Households, Aged 25-64 in 1970, with no missing Education or Earnings data for 1970, and in PSID sample all five years.*

Sample J: Omits respondents with missing data in any year.

Sample K: Omits cases with zero or negative five-year average earnings or any missing data.

Sample L: Omits cases with zero or negative earnings in any year or missing data any year.

Sample M: The subset of Sample L who were 35 to 54 in 1970

	Sample I		Sample J		Sample K		Sample L		Sample M	
Weighted N	2228		2099		2063		1945		1099	
Unweighted N	2202		2081		2036		1890		1088	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Education	11.6253	3.6658	11.5744	3.6619	11.6245	3.6363	11.7540	3.6025	11.7618	3.7528
Earnings										
1967			9047	5569			9472	5413	10203	5515
1968			9409	6124			9908	5971	10574	5742
1969	9925	6940	9661	6169	9826	6090	10171	5940	10870	6263
1970			9711	6769			10293	6539	10866	6349
1971			9642	6854			10291	6583	10998	6362
5 Year Average	9725	6471	9494	5859	9657	5775	10027	5681	10702	5680
Ln 1969 Earnings							9.0755	.5851	9.1326	.5494
Ln 5 Year average					8.9907	.7301	9.0791	.5342	9.1574	.5063

Regression Education & Earnings	Unstandardized Coefficient	R^2	B	R^2	B	R^2	B	R^2	B	R^2
1969 Earnings	856	.20456	776	.21197	763	.20746	753	.20850	861	.26615
5 Year Average	857	.23577	780	.23738	767	.23355	751	.22656	823	.29573
Ln 1969 Earnings							.07791	.23010	.08057	.28774
Ln 5 Year Average					.09197	.20978	.07688	.26883	.07868	.34009

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* Earnings figures from Sample I contain some allocated values for 1967, 1968, 1970, and 1971 Earnings.

for earnings. This table covers only men who are part of PSID Sample E and who were heads of a household that participated in the survey all five of the years from 1968 through 1972. Thus, men who entered the PSID sample as heads of splitoff households in 1969 or 1970 have been eliminated. (Men who died or dropped out of the sample between 1970 and 1972 have already been eliminated from the 1970 Basic Comparison Sample because I had to use 1972 weights.) The other way in which this table differs from preceeding ones is that the earnings figures shown are derived from the original earnings estimation algorithm rather than my modification (see Part 2 above).[28/]

Some other technical points about the construction of this table are worth noting. I used the Consumer Price Index to deflate income amounts from other years to 1969 dollars before taking the five-year average. This insures that inflation will not cause later years to have undue weight in the average. I also included men in the sample who were 25 to 64 in 1970, even if they were younger than 25 in 1968 or were older than 64 in 1972. Consequently, the average earnings figure covers years in which the men in the sample were more likely to be either students or retirees than they were in 1969. I took the log of the arithmetic mean for the full period, not the mean of the annual logs.

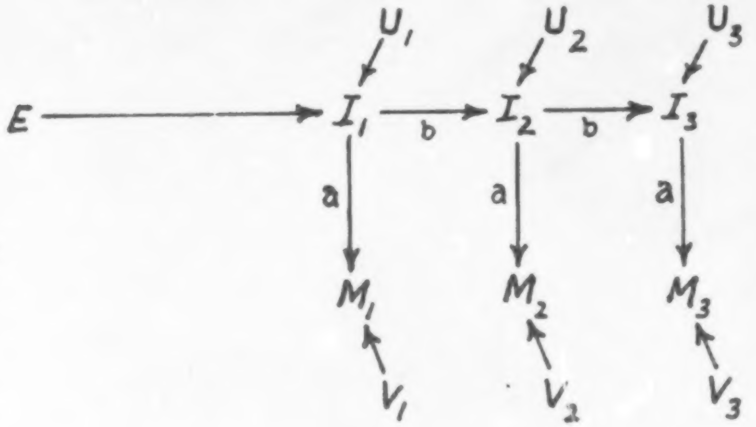
[28/] This consistently overestimates earnings for men who had self-employment losses and for men with both substantial self-employment income and income from wages or salary. While the difference is unimportant in terms of the earnings mean for the overall sample, it does directly affect some of those men whose income might be expected to be most variable.

I have also omitted from the table any statistics that would have required me to estimate Ln Earnings for men with zero or negative earnings.

My reading of Table 16.13 is that while R^2 increases over longer accounting periods, only part of the effect is due to averaging. The rest is due to sample restrictions. The columns in Table 16.13 show progressively more restricted samples. Sample J deletes men with missing data. Sample K deletes men with zero total earnings. Sample L deletes men with zero or negative earnings in any of the five years. Sample M deletes men outside the 35 to 54 age range in 1970. For male heads of households between the ages of 35 and 54, with complete data from 1968 to 1972, and with positive earnings for all five years (Sample M), R^2 in the regression of Ln Average Earning on education reaches an impressive 0.34. But R^2 is almost 0.29 for the regression of Ln Earnings for a single-year (1969) on education in this sample. Other comparisons are similar, except that averaging earnings raises R^2 even less for the less restricted sample and for dollar earnings.

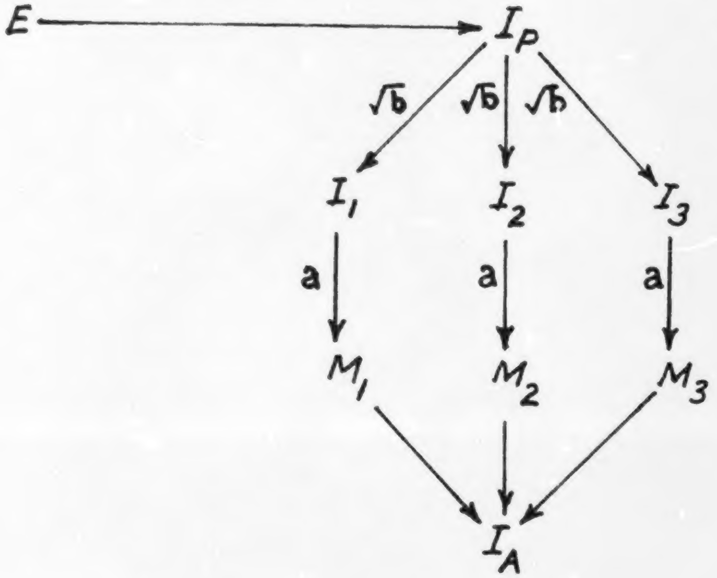
The magnitude of the increment in R^2 due to averaging over the five year accounting period is very much the same as the magnitude of my correction for attentuation due to unreliability. (See Part 4 Above.) I used a reliability estimate based on a simple Markov model and intercorrelations for earnings from adjacent years of the PSID. For instance, taking 1969 Ln Earnings in Sample M this model produces a reliability estimate of 0.875. The standard correction for attenuation is to divide R^2 by the reliability (ignoring, at this point, any correction for unreliability in the education variable). Corrected R^2 is thus 0.28774 / 0.875 = 0.329 or about 0.01 less than the R^2 figure for the five-year average. For dollar earnings in Sample K the correction for attenuation yields an R^2 of 0.20746 / 0.880 = 0.234 compared to the R^2 for averaged earnings of 0.236.

This result should not be too much of a surprise since the mathematics of the two operations are closely akin. The main difference in the structural models that underlie the two processes is the insertion of "permanent income" into the averaging model. The simple Markov model behind the corrections for unreliability looks like this:



Where the I_j stand for true income in consecutive years, the M_j are measured incomes, and the U_j and V_j are uncorrelated error terms , and E is Education. In this model a^2 is equal to the coefficient of reliability of measured income

The model implied by income averaging adds Permanent Income (I_p) and Averaged Income (I_A). Omitting the error terms it looks like this:



As observations of income in subsequent years are added to this model (I_4, I_5, etc.), the value of Averaged Income will approach Permanent Income, because the effects of unreliability (the fact that path a is less than 1.0) will be averaged out, as will the effects of true income variations from year to year ($\sqrt{b} < 1.0$). Corrections for attenuation will raise R^2 less because they only deal with the effects of path a.

For samples of men in mid-career and fairly short income accounting periods, the assumption of a hypothetical permanent income is likely to be an attractive one. Under these conditions averaging is an easy way to proceed. There is probably some conceptual value, however, in making the model explicit.

9 Does Identity of Informant Make a Difference for Earnings Responses?

My data also allow me to explore the effect of interviewing another member of the household instead of the targeted respondent. This is probably not an important issue for comparing the 1970 Census to the PSID survey, but it looms larger in comparing these results to other important surveys, such as the CPS series. SRC interviewers succeeded in talking to the head of the sample household about nine times out of ten. The directions that accompanied the 1970 Census requested that "the householder" fill out the form; it seems a reasonable assumption that most of the males aged 25-64 years in my sample, i.e. males whom the Census defines as household "heads, filled the questionnaire out for themselves. But we have no way of actually knowing what proportion of the sample actually did this. For surveys like the CPS, on the other hand, where interviewers will talk to whomever happens to be home, most of the information about the head of household's finances is supplied by the wife or some other household member.

The wife or other family member answering questions about the head's income will usually be worse informed about his financial affairs than he is, but it is not at all clear how her errors will affect the overall distribution of responses or the relationship of the income variable to other variables. If the errors are mostly random, the mean of the income distribution will be unchanged. If the wife often omits a secondary source of income, the income mean will be biased downward. If some wives, in ignorance of the true state of affairs, tend to paint too rosy a picture, upward bias will result. The standard deviation of the income distribution could be biased upward if most errors were random. It would be biased downward if most errors were in the direction of the mean, i.e. if errors were negatively correlated with true values. One plausible notion is that wives would tend to exaggerate the consistency between their husband's income and other aspects of his position that they know more about, such as his occupation or education. Such errors could increase the correlation between education and income. Purely random errors would lower it.

Unlike the Census, the PSID allows one to determine whether or not the head of household was the actual informant in each survey year. Table 16.14 divides the PSID respondents into three groups. In 81 percent of the sample the head answered the questions for himself in all five of the years. This is Sample N. In 2 percent of the sample the wife or another household member answered questions in all five years (Sample O). In the remaining 17 percent of households, the head answered the questions for himself in at least one, but not in all five of the survey years (Sample P).[29]

Comparison of Sample O to Sample N might suggest that wives are optimistic in their reports of their husbands' earnings, that their errors are correlated with their husbands' educations, and that they almost never admit to their husbands' having extremely low earnings. But such an interpretation is too simple. The lower education mean of Sample O is more than accounted for by the difference in average age between Samples N and O. There is no reason to believe that the wives' earnings reports are unreasonable for this tiny subsample. On the contrary, the failure of SRC's determined attempts to interview the heads personally suggests that they were too busy making money to waste time talking to researchers. The wives of Sample O report that their husbands worked an average of 2346 hours per year over the five year period, compared to 2175 hours for Sample N and 2230 for Sample P.

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[29] Like Table 16.13 this table uses a sample of continuous heads all five years and an Earnings computation that overestimates the Earnings of some self-employed men. Men with missing Earnings data in any year have been dropped from the table. This shrinks the size of the wife-informants sample, since there is a substantial overlap between missing data and nonhead informant.

Sample P is more instructive, since here the two sets of informants are reporting on the same individual's earnings, although in different years. These heads answered for themselves an average of 3.3 out of the five years, so the means based on head's responses are slightly more reliable than means based on the wive's responses. Sample P is less well off than Samples N and O in every respect, with lower earnings, lower education in spite of its relative youth, and a greater likelihood of being Black or Spanish American.

The weighted mean of the wives responses ($9186) is about $400 higher than the weighted mean of the heads' responses ($8767), but part of the difference is due to a small positive correlation between how many years the wife is the informant and the husband's earnings (as reported by himself). Heads whose wives often report for them. also tend to be older and to have more education. In other words, they tend to resemble the respondents in Sample O.

Table 16.14

Does identity of informant affect responses? Education and earnings statistics for PSID male heads of households, aged 25-64 in 1970, interviewed all five years (1968-72), with no missing education or earnings data any year.

Sample N: Head is informant all five years
Sample O: Someone else (usually wife) informant all five years
Sample P: Head is informant 1 to 4 years, someone else, the other years

	Sample N		Sample O		Sample P				
Weighted N	1696		14		359				
Unweighted N	1629		33		419				
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.	
Education Mean and S.D.	11.7344	3.5841	11.4326	4.3191	10.8355	3.8557			
Mean Earnings (1968-72 in 1969 $)	9556	5759	11905	7116	8907	6082			
Age (1970)	43.5678	11.0671	47.3099	10.5647	41.2995	10.6126			
% White	88.53		86.24		84.36				
Taxable Family Income 5 yr. avg.	9907	6150	12322	7593	9157	6256			
Annual Earnings (in 1969 $)					Head's Responses		Wife's Responses		Sample P Percentage of head responses
1967	9149	5668	11074	5677	8093	4470	8822	5919	68.98
1968	9484	5950	11150	5366	8230	4706	10424	10599	72.16
1969	9739	6115	11723	7915	8802	6382	9495	5585	66.15
1970	9761	6474	12510	9138	9364	8306	8715	6388	63.67
1971	9644	6549	13068	9682	9488	8494	8739	6132	63.02
Regressions of Education on Earnings	B	R^2	B	R^2	B	R^2	B	R^2	
1967	665	.17663	896	.4644	689	.35312	552	.12921	
1968	724	.19028	799	.41405	639	.27409	1099	.15973	
1969	772	.20449	1262	.47408	646	.15238	878	.36739	
1970	833	.21287	1240	.34375	860	.15931	708	.18235	
1971	880	.23181	1224	.29809	901	.16714	752	.22312	
5 yr. total	775	.23244	1084	.43307	759	.23137			

There is some evidence that wives' responses have a higher level of random error than heads' responses. The correlations between heads' responses in consecutive years average to 0.846, while the average correlation for wives is 0.802. The average of the correlations between a head's response in one year and his wife's in the next (and vice versa) is 0.815. In each of these cases, however, the range of correlations is much greater than the difference between their averages. Wives' reports may be a little less accurate, but there is no indication from these data that reliance on wives as informants has any marked effect on survey results for education and earnings.

10 Some Sample Restrictions and their Effects

Table 16.16 presents education and earnings results for samples with several restrictions in addition to the age, sex, head of household, and complete-data restrictions that defined Sample E. The restrictions I test here--race, student or military status, farm background, narrower age limits--are some that have been used elsewhere in this book and by other investigators. The questions used to impose the restrictions appear in Table 16.15.

One of my objectives is to show how each of these restrictions, taken individually, modifies my results. Because each of the groups being

Table 16.15

Wording of Questions, Coding, and Frequency Distributions of Responses for Variables Used to Restrict Samples. Comparison of Sample E from the 1970 Census and 1970 Wave of the PSID: Male Heads of Households, Aged 25-64 in 1970, with Complete Data on Age, Sex, Household Status, Education and 1969 Earnings. (Census N = 32549; PSID N = 2338; Unweighted N = 2349)

Variable	Question	Coding	Computer Code	Frequency
Nonstudent (Census)	Since February 1, 1970, has this person attended regular school or college at any time? Count nursery school, kindergarten, and schooling which leads to an elementary school certificate, high school diploma, or college degree. (Q 20)	No	1	97.6
		Yes, public	0	2.4
		Yes, parochial	0	
		Yes, other private	0	
				100.0%
Nonstudent (PSID) (Version S1)	Are you (HEAD) taking any courses or lessons? What are they? (J2, J3, 1970)	Formal academic - college, high school	0	3.8
		Potential learning applications	1	4.4
		Do-it-yourself courses	1	0.3
		Increasing knowledge about the world	1	0.3
		Increasing consumption pleasure	1	1.0
		N.A. What	1	0.1
		Inappropriate	1	90.1
				100.0%
Nonstudent (PSID) (Version S2)	1. Now we would like to know about your (HEAD'S) present job -- are you (HEAD) working now, looking for work, retired, a housewife, or what? (D1, 1969)	1969 Question		
		Working now	1	94.2
		Unemployed	1	.9
		Retired	1	4.0
		Housewife	1	.2
		Student	0	.6
		Other	1	.1
				100.0%
	2. We would like to know about your (HEAD'S) present job -- are you (HEAD) working now, looking for work, retired, a housewife, or what? (D1, 1970)	1970 Question		
		Working now	1	93.6
		Unemployed	1	.9
		Retired	1	4.5
		Housewife	1	.1
		Student	0	.9
		Other	1	.1
				100.0%

Table 16.15 continued

Variable	Question	Coding	Computer Code	Frequency
Nonmilitary (Census)	Current or most recent job activity. Describe clearly this person's chief job activity or business last week, if any. If he had more than one job, describe the one at which he worked the most hours.	Employment Status		
		At work	1	87.4
		With a job-not at work	1	2.3
		Unemployed	1	2.1
		Armed Forces at work	0	1.5
	1. Industry	Armed Forces on leave	0	.1
	a. For whom did he work? If now on active duty in the Armed Forces, print "AF" and skip to question 36. (Q33)	Not in labor force	1	6.6
				100.0%
Nonmilitary (PSID)	What is your main occupation? What kind of business is that in? (D2, D3a, 1971)	Industry of main occupation		
		Armed Forces	0	.9
		32 other categories	1	99.1
				100.0%
White (Census)	Color or race. Fill one circle. If "Indian (American)," also give tribe. If "Other," also give race.	White	1	91.4
		Negro or Black	0	7.5
		Indian (Amer.)	0	
		Japanese	0	
		Chinese	0	1.2
		Filipino	0	
		Hawaiian	0	
		Korean	0	
		Other	0	
		No Answer* (N=286)	1	100.0%
White (PSID)	By observation only. Race? (M3, 1970)	White	1	87.8
		Negro	0	8.7
		Spanish American	1	2.8
		Other	0	.7
				100.0%
Father Not a Farmer (PSID)	What was your father's usual occupation when you were growing up? (L2, 1970)	None	1	.1
		Professionals	1	5.8
		Managers	1	3.9
		Self-employed	1	6.6
		Clerical, Sales	1	4.1
		Craftsmen	1	20.6
		Operatives	1	15.1
		Laborers, Service, Farm Laborers	1	9.1
		Farmers and Farm Managers	0	26.7
		Miscellaneous (incl. no answer)	1	8.0
				100.0%

Table 16.16

Effects of some common sample restrictions: means, standard deviations and correlations for 1969 earnings, ln earnings and 1969 personal income with years of education. Census and PSID Male Heads of Households Aged 25-64 in 1970 with Complete Data on Education, Earnings, Age, Sex, and Household Status with additional restrictions as shown.

Sample V: Combined Restrictions - White, Non-student, Non-military, and Earnings > 0

Sample W: Combined Restrictions (as in Sample V) with additional restriction to Age 35 to 54.

	Whites Only		Nonwhites Only		Nonstudents Only		
	CENSUS	PSID	CENSUS	PSID	CENSUS	PSID	PSID
	Sample Q		Sample R		Sample S	Sample S1	Sample S2
Weighted N	29731	2118	2818	219	31769	2250	2310
Unweighted N		1657		736		2271	2322
Education Mean	11.6721	11.8887	9.7031	9.5308	11.4293	11.5664	11.6434
Education S.D.	3.4948	3.5088	3.9791	3.9559	3.5648	3.6201	3.6207
Earnings Mean	9652.11	10156.73	5931.46	6765.10	9367.43	9871.85	9912.86
Earnings S.D.	7614.73	6955.75	4735.26	4068.92	7525.06	6885.17	6809.07
Earnings-Education Cor.	.35964	.42214	.36954	.56252	.38195	.45314	.45124
Unstandardized Reg. Coef.	783.61	836.84	439.77	578.59	806.28	861.84	848.59
R^2	.12934	.17820	.13656	.31643	.14589	.20533	.20362
Ln Earnings Mean							
Ln Earnings S.D.							
Ln Earnings-Education Cor.							
Income Mean	10159.81	10870.90	6194.16	7191.92	9852.77	10569.67	10587.19
Income S.D.	7963.35	7376.22	4760.94	4098.64	7862.40	7303.26	7228.97
Income-Education Cor.	.36287	.41032	.36854	.56441	.38409	.44213	.43767

	Nonmilitary Only		Non Farm Father	Combined Restrictions		Combined Restrictions and 35 to 54 Age Limits	
	CENSUS Sample T	PSID	PSID Sample U	CENSUS Sample V	PSID	CENSUS Sample W	PSID
Weighted N	32026	2317	1714	26917	1958	14699	1066
Unweighted N		2326	1591		1520		825
Education Mean	11.4700	11.6536	12.1240	11.6943	11.8542	11.7642	11.9556
Education S.D.	3.5860	3.6245	3.4620	3.4184	3.4872	3.4142	3.5841
Earnings Mean	9348.28	9840.66	10382.76	10199.62	10514.55	10967.84	11495.04
Earnings S.D.	7522.13	6835.57	6870.58	7583.09	6932.11	8113.94	7433.11
Earnings-Education Cor.	.37297	.44220	.44464	.35994	.42781	.39625	.47107
Unstandardized Reg. Coef.	782.36	833.97	882.42	798.46	850.43	941.71	976.94
R^2	.13911	.19554	.19771	.12956	.18302	.15701	.22191
Ln Earnings Mean				9.0284	9.0713	9.1038	9.1745
Ln Earnings S.D.				.6881	.6837	.6756	.6443
Ln Earnings Education Cor.				.38057	.44315	.42066	.49338
Income Mean	9838.43	10529.76	11034.93	10603.79	11168.20	11368.55	12124.95
Income S.D.	7856.96	7240.65	7128.22	8020.51	7473.90	8529.40	7863.87
Income-Education Cor.	.37613	.43156	.44030	.36343	.41781	.40112	.46767

eliminated has a distinctive relationship to the job market, there are reasonable theoretical grounds for dropping them from many analyses. Racial minorities, students, military men, and rural respondents are also exceptionally difficult to find and interview. Surveys therefore differ in their coverage of these groups.

The differences between samples that include non-whites and samples restricted to whites have been well-documented elsewhere. White samples have higher education and earnings means, often slightly lower correlations between education and earnings, and negligible differences in unstandardized regression coefficients. A comparison of the Sample Q in Table 16.16 to Sample E in Table 16.8 shows this to be true of both the Census and PSID surveys. There is significant difference in the proportion of non-whites in the two surveys (Census, 8.7 percent; PSID 9.4 percent). One reason that the proportion of non-whites is slightly lower in the Census is that the rate of missing earnings data for nonwhites (about 20 percent) is twice as high as the rate for whites (about 10 percent). For samples that include respondents with missing earnings data, the PSID and Census proportions of non-whites are virtually the same. 30/

While there is no evidence that the proportions of non-whites differ, there is some evidence of systematic differences between the non-white respondents found by the Census and the PSID. Sample R in Table 16.16 gives education and earnings statistics for non-white in the two surveys. The education-earnings correlation for PSID non-whites is remarkably

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30/ Unlike Mueser (Appendix D) and Schwartz and Williams (Chapter 8) I have treated PSID Spanish Americans as white. This maintains comparability between PSID and Census results.

high (0.56). Because of the deliberate over-sampling of low income respondents in the PSID, this statistic is based on a sample large enough to give fairly stable results. As I noted in Part 2, however, many of these respondents came from an OEO sample interviewed in 1966 and 1967. If panel designs cause selective attrition, the effects would be more serious for this group than for the rest of PSID sample. Eliminating non-white respondents from the sample reduces the difference in R^2 between the PSID and the Census from about 0.06 to about 0.05.

The samples labelled with an S in Table 16.16 try to omit student respondents. The only student filter question available from the Census asks about the respondent's school attendance at the time of the survey in the spring of 1970. Undoubtedly, some of the respondents who attended school during the previous year had gone back to work by the time of the survey, whil others had been working the whole previous year, but had just gone back to school; the language of the question allows part-time school attenders to answer "yes," as well. All in all, the Census question is not an ideal instrument for locating respondents whose 1969 earnings were depressed because of school attendance. With the PSID survey, there was a choice of student filter questions. The one for sample S1 is even broader in its language than the Census question; it identifies respondents who are taking formal academic courses at the time of the survey (Spring, 1970). The questions on employment status (Sample S2) are more likely to isolate full-time students from part-time students. To catch respondents who had been students in the 1968-1969 academic year as well as the 1969-1970 academic year, I coded the S2 filter to exclude respondents who had reported being students at the time of the 1969 interview, whether or not they reported student status in 1970. The Census question designated about 2.4 percent of Census Sample E as students. PSID Sample S1 excludes 3.8 percent of Sample E as students; Sample S2 excludes 1.2 percent.

Whatever the filter question, the omission of students generates small increases in the mean for earnings and the unstandardized regression coefficient of education. Excluding students raises R^2 by about 0.01. Although the PSID Version II filter excludes a much smaller percentage of the sample, the effects of the exclusion on mean earnings are about twice as large as with the broader filter. The effects on R^2 are comparable to those for Version I. My findings are probably upper-bound estimates of the effects of omitting students, since all my samples here include zero-earners. But if the age limits were changed to include men younger than 25, the treatment of students would loom larger.31/

Removing military men from the sample (Sample T of Table 16) has only a very limited effect on the earnings and education statistics for the overall sample. The PSID sampling frame does not cover military men living on base, while the Census does. Not surprisingly, a larger proportion of the Census sample than the PSID reports being in the Armed Forces (1.6 percent to 0.9 percent). The effects of excluding military men are, correspondingly, a little larger for the Census than the PSID, but the changes are negligible for both surveys.

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31 _/ See Weiss and Williamson (1975) on the effects of student and military restrictions on samples that include younger men.

Sample U in Table 16.16 is a PSID sample that omits respondents whose fathers were farmers. (The Census questionnaire does not ask about Father's Occupation.) Earnings and Education means are substantially larger for this nonfarm sample, and the unstandardized regression coefficient of education shows a moderate increase (about $50). The earnings-education correlation, however, is virtually the same for this sample as for a sample that includes men with farm backgrounds.

Samples V and W in Table 16.16 combine a number of the restrictions from previous samples. Sample V excludes non-whites, students (S1 version for PSID), military, and zero or negative earners from Sample E. These restrictions together whittle away at the differences between the Census and PSID, even in comparison to Sample F (Table 4B) that excludes zero earners. The gains are only minor, however--20 percent or less of the gap between the surveys for the statistics covered here. Sampling problems related to coverage of non-whites, students, and military men bear part of the responsibility for the survey differences, but have less impact than differences in coverage of zero earners and nonheads.

Sample W is the subset of Sample V who were between the ages of 35 and 54 in 1970. I include this as an illustration of the way in which age restrictions can modify these results. Sample W covers men at the peak of the earnings cycle, so their mean earnings are almost $1000 higher than for Sample V. Correlations with education and unstandardized regression coefficients are higher, as well. The increase in regression coefficients indicates that a year of schooling is worth $100 more for this sample in both surveys. The percentage of variance in Earnings 1/3 explained by education exceeds 20 percent for Census Sample W and 27 for PSID (not shown here). Education has more important effects on the earnings of men at the height of their careers than on the earnings of older or younger men.

Summary and Conclusions

I sympathize with the reader who is feeling confused at this point by the parade of numbers in this chapter, each one only slightly different from the last. I know the feeling well. Table 16.17 is an attempt to reintroduce some perspective by returning to one of the questions I began with: how much leeway does a researcher have for making substantive changes in his results by fiddling around with the methodological details? In Table 16.17 I have gone back through the preceeding tables to pick out the largest and smallest versions of the education and earnings results I found for the Census and PSID. In the next few paragraphs I will review which of the methodological changes were most important in producing the range of results for each type of statistic in Table 16.17.

Education means. The difference between the largest and smallest of the education means is rather small--only about half a year for the PSID and less than that for the Census. Sample restrictions were mainly responsible for the changes. The elimination of men whose fathers were farmers, of non-whites, of zero earners, and of non-heads of households all tend to increase the education mean. Education is more equally distributed in the adult male population than earnings, so the leeway for producing dramatic changes in the mean is smaller for education than for earnings. Since mean educational attainment has been steadily increasing, restricting a sample to young men or old men will produce fairly large changes in the mean.32/

32/ See Robert M. Hauser and David L. Featherman, 1976, "Equality of Schooling: Trends and Prospects," Sociology of Education 49: 99-120.

Table 16.17

Summary of the Largest and Smallest Values I Report in Preceeding Tables for Selected Statistics from the 1970 Census and 1970 Wave of the PSID

Statistic				Table Location (Identifying Information)			
				CENSUS		PSID	
		CENSUS	PSID	Table	Sample	Table	Sample
Education Mean	Largest	11.7642	11.1240	16	W	16	U
	Smallest	11.3747	11.5847	3	A	6 (original coding)	E
Earnings (or Income) Mean	Largest	11369	12125	16 (Income)	W	16 (Income)	W
	Smallest	8838	9803	3	A	8 (new calculation)	E
Earnings Regression Coefficient	Largest	942	977	16	W	16	W
	Smallest	745	751	3	A	13 (5 year average)	L
Earnings R^2	Largest	.16090	.29573	16 (Income)	W	13 (5 year average)	M
	Smallest	.12527	.17793	9 (original code)	F	12	H
Ln Earnings (or LnIncome) Mean	Largest	9.1038	9.1745	16	W	16	W
	Smallest	8.3202	8.7612	10 (version I)	E	8	E
Ln Earnings Regression Coefficient	Largest	.19350	.14338	10 (version I)	E	8	E
	Smallest	.07571	.07688	10	F	13 (5 year average)	L
Ln Earnings R^2	Largest	.17695	.34009	16	W	13 (5 year average)	M
	Smallest	.05335	.09472	10 (version 2)	E	8	E

Earnings and Income Means. Income means from the same survey can be made to differ by as much as $11369 - $8838 = $2531. Sample restrictions are among the most important causes of the differences. Restrictions by age categories, head of household, race, non-zero earnings, and non-farm fathers all have a substantial impact on the mean. For my samples of mature men, omission of students or military men has little effect on income means, but it would make a difference for samples that included men under 25. Other methodological decisions also have an impact on mean income. The CPS's broad income categorization usually results in substantial upward bias of income means. The inclusion of unearned income as well as earnings in the income variable will also make a difference of several hundred dollars in income means.

Unstandardized regression coefficients of earnings on education in general. Methodological changes that increase income or earnings means also tend to increase the standard deviation and the regression coefficient of education, and the changes are proportionately larger than changes in means. An exception to this rule seems to be the use of multi-year accounting periods, which, because of the accompanying sample restrictions, tends to make regression coefficients lower than for unrestricted samples, although the income means increase.

R^2 for earnings and education regressions. For dollar earnings equations, R^2 shows less variation across methodological treatments than other statistics. Multi-year averaging, especially eliminating those with no earnings in any year, does cause substantial increases in R^2.

Ln Earnings Means. For statistics derived from the log transformation of earnings, the crucial decision appears to be what to do about zero earners. Including zero earners in the sample will dramatically lower means, the extent of the changes depending entirely, as I have shown, on the arbitrary choice of a value for the logarithm of zero earnings.[33]

Less dramatic changes in Ln Earnings means can be caused by sample restrictions, especially those that narrow the 25 to 64 age range or in other ways exclude men with very low earnings.

Ln Earnings Unstandardized Regression Coefficients. The inclusion of zero earners biases regression coefficients upward quite dramatically. In general, Ln Earnings regression coefficients increase with the number of low earners in the sample.

R^2 for Ln Earnings and Education. R^2, on the other hand, tends to get smaller as the number of low earners in the sample is increased. The most dramatic examples are for samples that include zero earners. Coding changes that restrict the income range and multi-year averaging tend to increase R^2 more for Ln Earnings than for dollar earnings.

What then are the methodological manipulations that have the most pronounced effect on earnings and education results for the surveys I have investigated?

1. Sample restrictions, e.g., age limits, restriction to heads of households, and deletion of zero earners, cause the largest changes in means and variances.

2. Multi-year income averaging, when accompanied by a number of restrictions in the sample, makes the largest difference in correlations.

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[33] Another strategy for dealing with this problem is to use a "displaced lognormal" transformation that involves the addition of a constant to the distribution prior to taking the logarithm. See Charles E. Metcalf, An Econometric Model of the Income Distribution, Chicago: Markham, 1972.

3. Substitution of Personal Income for Earnings, has substantial effects on income means and regression coefficients, but relatively minor effects on correlations.

4. Some kinds of coding choices, e.g. broad-category coding of income and assignment of a code for log of zero earnings, make a substantial difference in results.

The other major question I have been trying to answer in this chapter is whether or not differences between PSID and Census results can be erased by the methodological manipulations at my disposal. The answer is a qualified no. Table[16.17] shows considerable overlap in the ranges of Census and PSID statistical results, except for R^2. However, my efforts to impose parallel sample restrictions, variable definitions, and coding remove less than half of the statistical gap between the two surveys. The remaining differences may or may not be statistically significant, since PSID standard errors are only approximate because of the survey design and weights. I have shown that plausible corrections for attenuation due to measurement unreliability have little effect on the divergence in R^2 for the Census and PSID. I have also argued that non-reporting of Census earnings may account for some of the difference in earnings means, but not in R^2. My comparison of PSID and NLS results demonstrated that the panel design of the PSID, per se, is not responsible for the divergence of the PSID from other surveys. There is, finally, the hypothesis of a survey organization effect (See Jackson, Chapter 15). The reasons for such an effect remain opaque.

In any event, my results suggest that researchers comparing findings from different surveys should pay close attention to sample restrictions, variable definitions, and the peculiarities of sample design.

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